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# How Long Did They Go? Prospectively Paid Agencies Continue to Reduce Service Use

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*Final Report*

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# EXECUTIVE SUMMARY

The Per-Episode Home Health Prospective Payment Demonstration of the Health Care Financing Administration (HCFA) tests the extent to which prospective payment for Medicare home health services increases efficiency in service provision. Under the demonstration, participating home health agencies are paid a fixed, lump-sum payment for the first 120 days of each episode of care provided to Medicare beneficiaries, and a predetermined rate for each visit thereafter. By allowing agencies to retain most of any surplus payments over cost, this payment method gives agencies an incentive to provide home health care in a cost-efficient manner.

In an earlier analysis of service use during the first two years of the demonstration, we found that prospective payment reduced the average number of visits to patients by shortening the overall length of service and by lowering the frequency of visits provided. The present report examines service use through all three years of the demonstration. We find that prospective payment continued to have a significant impact on both the number of visits provided and the length of service.

## BACKGROUND

Ninety-one agencies in five states entered the three-year demonstration at the start of their 1996 fiscal year. Prior to the start of the demonstration, the participating agencies were randomly assigned to either the treatment group or the control group. Agencies assigned to the treatment group were reimbursed under the demonstration's prospective payment method, while those assigned to the control group continued to be reimbursed under Medicare's method of cost-based reimbursement (the payment method for all home health agencies when the demonstration began).

For each patient admitted to home health care, treatment group agencies received a lump-sum payment for the first 120 days of home health care, regardless of the number or cost of visits provided. The agencies were thus "at risk" for the costs of care incurred during this period. Only after the 120-day at-risk period, and after a 45-day gap in services had elapsed, was an agency able to receive a new per-episode payment for a given Medicare beneficiary. For each visit beyond 120 days that did not begin a new episode (referred to as the "outlier period"), treatment agencies received a fixed payment rate that varied by type of visit.

To counteract the incentive to dramatically reduce services at the expense of quality, as well as prevent agencies from realizing excessive profits at public expense, HCFA shared in any profits above selected levels. HCFA also provided a loss-sharing arrangement as a means of encouraging agencies to participate in the demonstration.

## RESEARCH QUESTIONS AND METHODOLOGY

In this report, we use the data from all three years of the demonstration to test various hypotheses about the effects of prospective payment on the provision of home health services. These hypotheses relate to the impact of prospective payment on (1) the total number of visits provided, (2) the number of visits by type, (3) the probability of receiving certain types of visits, and (4) episode length.

We measure impacts on service use during the first 120 days after admission (the "at-risk period"), during which all service use falls under episodic payment. Although this time period is considerably shorter than the 365-day ("patient-year") period we previously used to examine service use, it allows us to determine whether the impact changed over the course of the demonstration. Our analysis focuses only on the first admission for a patient taking place during the demonstration. By doing so, our impact estimates are unaffected by the possible correlation between treatment status and subsequent episode starts, and we avoid double-counting patients.

The methodology for the present report is similar to that used in our previous report. The sample for the main analysis included 50,391 patients admitted to one of 74 demonstration agencies in Year 1, 39,633 patients admitted in Year 2, and 24,092 patients admitted in Year 3. Medicare claims files provided data on the outcome variables describing patients' use of services following admission. Ordinary least squares and logit models were used to estimate program effects, controlling for preexisting differences between treatment and control agencies in patient and agency characteristics. Observations were weighted so that each agency was represented equally in the analysis. Analyses of the robustness of our regression estimates showed that they were not sensitive to the weighting scheme, statistical methods, or model specifications used.

Finally, we also provide a descriptive analysis of long-stay patients; patients who receive care for one year or more. Our results from that analysis, however, are very sensitive to the weighting schemes, and thus no firm conclusion can be drawn.

## FINDINGS

The impacts of the demonstration remained constant during the three-year demonstration. In the first year, prospectively paid agencies provided an average of 37.6 visits during the at-risk period, while the cost-reimbursed agencies provided 45.3--for an impact of 7.7 visits. By Year 3, the prospectively paid agencies reduced their service use to 32 visits per episode. Cost-reimbursed agencies also reduced their service use to 41 visits in response to such factors as the growth of managed care; the elimination of venipuncture as a qualifying skill need for Medicare home health benefits; Operation Restore Trust, the Interim Payment System; and the anticipation of prospective payment.

The impact on total visits is driven primarily by reductions in skilled nursing and home health care visits, regardless of the time period. During Year 1, prospectively paid agencies provided 18 skilled nursing visits per at-risk period--almost 4 visits fewer than the 21.7 visits rendered by the cost-reimbursed agencies. By Year 3, prospectively paid agencies had reduced the number of

skilled- nursing visits to 15.6, while the cost-reimbursed agencies cut their visits to 20.8--resulting in a difference of just over 5 skilled-nursing visits. A similar pattern can be found in home health aide care--where, throughout the demonstration, prospectively paid agencies consistently provided about 3 visits fewer per at-risk period. In contrast, impacts on the number of therapy visits were small, insignificant, and changed little over the demonstration. Impacts on medical social worker visits were also unchanged.

The continued reduction in the number of visits was in part due to the continued decrease in episode length. By Year 3, 40 percent of the prospectively paid episodes ended within 30 days of admission--a 7 percentage point increase from Year 1. In contrast, by Year 3, 31.7 percent of cost-reimbursed episodes ended within 30 days of admission; this, however, is an 8.7 percentage point difference from Year 1.

Finally, our analysis of long-stay patients found that these patients continued to be a significant part of the agencies' caseload, representing 6.5 percent of all admissions to prospectively paid agencies and 35 percent of all visits; and that these patients were concentrated in particular agencies. Surprisingly, these are about the same proportions we found in the control group. Thus, even though prospective payment significantly reduced episode lengths, it did not reduce the importance of these long-stay patients. The observed characteristics of the long-stay patients remained the same--they were the same age, had the same primary diagnoses, and used the same amount of health care as the patients in cost-reimbursed agencies.

## LIMITATIONS OF THE REPORT

Despite the significance and robustness of our impact findings on the overall population, this report does have some limitations. Perhaps the most important potential limitation is the extent to which we can generalize our findings to home health agencies nationwide. Like any study design based on voluntary participation, the agencies in this demonstration may reflect the group best able (or most willing) to respond to the incentives of the intervention. If true, our demonstration impacts may overstate the actual reductions in service use that would take place nationally under a similar payment system. In addition, even if demonstration agencies had no particular predisposition to reduce the number of visits relative to similar agencies not participating in the demonstration, we may still overstate the impacts of prospective payment nationally because of other key differences between agencies in and out of the demonstration.

A second limitation, related to the issue of generalizability, is that a national program of prospective payment will differ from the one implemented for the demonstration. For example, agencies will not be protected from incurring financial losses under a national system, which could lead some agencies to respond more cautiously to program incentives.

A third limitation is that this report provides no information on the consequences of reducing services. Previous reports have already examined the effects of the demonstration on the quality of care and patient outcomes, patient selection and retention, and the utilization of other Medicare services. A final report will combine the findings across outcome measures.

A final concern is that we examined impacts over only a three-year period. While three years should be long enough for agencies to change their practices, our findings may not accurately reflect the long-term impacts of prospective payment. For the most part, however, the findings from this demonstration provide the information necessary to policymakers: within the first year of the demonstration, nearly all participating agencies in the treatment group made dramatic cuts in service use that may be attributed directly to the new payment system, and this continued throughout the demonstration. Thus, a key hypothesis that the demonstration was designed to investigate--whether prospective payment could curtail the tremendous growth in home health services--was strongly affirmed, despite the limited follow-up period.

## POLICY IMPLICATIONS

These findings demonstrate the potential advantage of a payment system that provides an *ongoing* incentive to reduce service use. In every year of the demonstration, prospectively paid agencies reduced their visits significantly from the year before. Presumably, these reductions took place over a long period of time as agencies discovered and adopted new ways to reduce service use without harming the quality of care. This flexibility to improve care delivery, combined with the ongoing financial incentive to do so, contrasts sharply with a system of direct regulation (such as the interim payment system) that merely offers short-term incentives to meet established levels of savings. While results from this demonstration do not allow for a comparison of these two systems of payment, they do show that a system which provides an ongoing incentive to improve care delivery will likely be successful.

One issue for designing the new national payment system is how to handle long-stay patients in Medicare home health. Although, over the past decade, long-term stay patients used a large proportion of the Medicare home health resources, they conceivably could become a minor part of the program once the financial incentives have changed. Although this demonstration does not offer as strong financial incentives as the national prospective payment system will, it does offer incentives to discharge patients early--and the prospectively paid agencies did so. We found, however, that, despite this incentive, long-stay patients remained about the same proportion of the caseload. Thus, as the national payment system is refined, attention to long-stay patients will continue to be an important issue.

# I. THE PER-EPISODE HOME HEALTH DEMONSTRATION AND EVALUATION

The Per-Episode Home Health Prospective Payment Demonstration of the Health Care Financing Administration (HCFA) tests the extent to which prospective payment for Medicare home health services increases efficiency in service provision. Per-episode payment encourages efficiency by giving agencies the incentive to reduce their costs. Specifically, under the demonstration payment system, any savings generated from lower costs per episode of patient care might result in agency profit. These incentives differ greatly from those under a system of cost-based reimbursement that provides no reward for cost containment.

In an earlier report (Trenholm 2000), we presented an analysis of service use during the first two years of the demonstration. In the current report, we will present our analysis of home health service use in the third year of the demonstration and compare service use across years one, two, and three. Specifically, we hope to learn whether the initial decrease in service use made by agencies in year one and additional cuts made in year two continued into year three. In addition, we will further analyze a previous finding that a significant percentage of home care patients receive services for more than one year. We will determine if there are treatment-control differences in the characteristics of this patient population. This additional analysis will provide information on the types of patients who will remain as long-term care patients under a prospective payment system.

## A. THE MEDICARE HOME HEALTH BENEFIT

After the settlement of a lawsuit against HCFA in 1988, coverage under the home health benefit broadened considerably, which, in turn, contributed to dramatic growth in Medicare home health expenditures after that time. This growth, combined with striking regional variation in service use and recent investigations of industry fraud and abuse, prompted Congress to legislate changes to the

Medicare home health benefit as part of the Balanced Budget Act (BBA) of 1997. The Act mandates implementation of a per-episode prospective payment system for Medicare home health.

## B. THE PER-EPISODE DEMONSTRATION

Ninety-one Medicare-certified home health agencies in five states--California, Florida, Illinois, Massachusetts, and Texas--enrolled in the three-year, per-episode demonstration. Of these, 48 were randomly assigned to the treatment group and received per-episode payment; the remaining 43 were assigned to the control group and continued to operate under cost reimbursement. Each agency entered the demonstration at the start of its fiscal year, with the earliest entrants to the demonstration beginning to implement prospective payment in June 1995 and the latest beginning in January 1996.

Agencies selected for the treatment group received a lump-sum payment for the first 120 days of home health care, regardless of the number or cost of visits provided. The agencies were thus "at risk" for the costs of care incurred during this period. Only after the 120-day at-risk period, and after a 45-day gap in services had elapsed, was an agency able to receive a new per-episode payment for a given Medicare beneficiary. For each visit beyond 120 days that did not begin a new episode (referred to as the "outlier period"), treatment agencies received a fixed payment rate that varied by the type of visit.

Prospective (per-episode) rates for the at-risk period were based on a treatment agency's costs, service mix, and case mix in the fiscal year preceding its entry into the demonstration (the base year), adjusted for inflation and changes in case mix in each evaluation year. To counteract the incentive to dramatically reduce services at the expense of quality, as well as prevent agencies from realizing excessive profits at public expense, HCFA shared in any profits above selected levels. HCFA has provided a loss-sharing arrangement as a means of encouraging agencies to participate in the demonstration.

Agencies selected for the control group received what, at the time of the demonstration's inception were the standard Medicare payments. That is, payments were based on agencies' actual per-visit costs, up to 112 percent of the mean cost incurred by all agencies (for the agency's mix of visits) in the same geographic area.

## C. SUMMARY OF PREVIOUS FINDINGS

In a previous report, Trenholm (2000) used the first two years of demonstration data to examine the effect of the demonstration on the use of Medicare-covered home health services. This report investigated several hypotheses, including: (1) the effect of prospective payment on the number of visits provided in the year after a patient's admission, both in total and by type; (2) the effect of prospective payment on the duration of services; (3) whether key impacts of prospective payment vary between selected subgroups of agencies or patients; and (4) whether key impacts changed over the course of the demonstration. In addition, we examined the length, frequency, and other characteristics of the episode of care under a prospective payment system.

The main analysis for this report centered on patients admitted to an agency during its first year in the demonstration, because it was possible to observe all their subsequent home health use over at least 12 months. Then, in supplementary analyses based on four and eight months of data, the key impacts for the first and second years of the demonstration were compared.

This report concluded that prospectively paid treatment agencies had reduced the average number of visits to a patient in the first year following admission by 24 percent, as compared to control agencies. Reductions in the number of visits occurred in both "at-risk" and "outlier" periods, and reductions were of a similar proportion for all the types of visits provided. Reductions were achieved both by shortening the overall length of service and by lowering the frequency of visits provided. In the second demonstration year, treatment agencies were able to make additional,

significant decreases in service use, compared to Year 1. These cuts, however, were matched by cuts in service use made by control agencies, resulting in stable impacts between Year 1 and Year 2.

The conclusions from the Year 1 and Year 2 analysis were that prospective payment generated large reductions in service use, shortened the length of the average patient episode, and did not appear to be highly "gameable." Additional reports from the demonstration, including that of Chen (2000), indicate that agencies were able to make these cuts in service use without jeopardizing the quality of care or patient outcomes and without causing increases in the amount of other Medicare or non-Medicare services used.

## D. SUMMARY OF CURRENT FINDINGS

This report presents findings that both confirm and extend the results of our previous report (Trenholm 2000). We found that treatment and control agencies continued to reduce both the number of visits they provided and the length of patient episodes in the third demonstration year. Demonstration impacts remained large and stable through to the end of the demonstration. We again conclude that a national prospective payment system based on the demonstration design would be feasible and effective in controlling the use of Medicare home health services.

In addition, we paid particular attention to those individuals who received home care services over an extended period of time--that is, more than one year. This population is of interest because these people account for a large percentage of total home health service use. Our analysis found that long-stay patients represented 6.5 percent of all admissions to prospectively paid agencies and account for 35 percent of the visits. This is about the same level as it was for the control group. Thus, even though prospective payment significantly reduced episode lengths, it did not reduce the importance of long-stay patients.

# II. DATA AND METHODS

This report contains two analyses: (1) an estimate of the impact of per-episode payment on the provision of home health services over the three years of the demonstration, and (2) a characterization of long-stay home health patients and a description of their service use. In the next three sections of this chapter, we provide information on the construction of the main file, the methodology used to arrive at the samples used in the impact analysis and the long-stay patient analysis, and summary statistics for the control variables. The final three sections then describe how we measure our outcome variables and summarize our statistical methods. Additional details on the data and statistical methods used to investigate service use during the demonstration are available in Trenholm (2000).

## A. CONSTRUCTING THE MAIN DATA FILE

The main data file was built from Medicare claims data extracted from HCFA's standard analytic files and contains a record of paid claims for all service use through December 31, 1998. This file was used to construct measures of home health utilization and several of the control variables measuring preadmission use of Medicare services.

### 1. Identifying Individual Patient Episodes (Records)

In order to compare service use for treatment and control group agency patients, we needed to construct patient episodes in a parallel way for both groups. For patients in both treatment and control agencies, we used data from UB-92 bill record files obtained from the demonstration fiscal intermediary, Palmetto Government Benefits Administrators (PGBA), to identify home health

episodes as defined by demonstration rules.[1] To identify the first admission for an individual and the complete set of that person's subsequent bill records, we scanned the UB-92 files, beginning the file search from the date of each agency's enrollment in the demonstration. The first demonstration admission determined the episode start date, from which we constructed the first record for the patient. To determine the end of the initial episode and the start of any subsequent episodes, we tracked bill records until we observed a 45-day gap in care that began after the end of the at-risk period (that is, after the first 120 days from admission). This procedure was followed regardless of whether the agency discharged and readmitted a patient within the first 165 (120 + 45) days after the initial admission. If we observed a readmission for a patient after 165 days, and a 45-day gap in care had taken place, we created another record for the patient corresponding to this second demonstration episode. We then repeated the process until we had constructed, for each patient, a series of records for all the episodes beginning in the agency between its demonstration start and the date corresponding to the last day of the eighth month in the agency's third demonstration year.[2]

## 2. Creating Outcome Variables

To construct the outcome variables, we used claims data from the standard analytic file created by HCFA.[3] After matching the episodes identified from UB-92 bill record data to the standard

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[1]We used the UB-92 bill records rather than HCFA claims data principally because they contain data on patient characteristics at admission. These data were then used to construct several key control variables.

[2]This end date was chosen so that admissions occurring in the last four months of the demonstration period would not be included. This ensures that we will have at least four months of data (covering the at-risk period) on every patient. For example, if an agency started the demonstration on January 1, 1996, then we would include in the data file admissions to that agency from January 1, 1996 through August 31, 1998.

[3]We chose to use HCFA's Standard Analytic File because it includes adjustments for any voided or amended bills. Although we could have used the UB-92 data and mimicked HCFA's adjustment algorithms, we could not have been sure that we would replicate all the adjustments perfectly.

analytic files, we extracted all claims for a given patient indicating that the admitting agency had provided home health care during the 36 months of the demonstration. Both Part A and Part B home health data were extracted.[4] The outcome variables that we constructed from this data are listed in Table II.1. As noted in the table, we measure these variables in several different ways.

### 3. Creating Control Variables

As discussed in Section II.E, the impact analysis relies on regression models that control for preexisting differences between treatment and control agencies. The control variables used in the main regression models are shown in Table II.2 and fall into one of two general categories: (1) patient characteristics, including basic demographics at admission and Medicare service use during the six months preceding admission; and (2) characteristics of the admitting agency and area in which the agency operates. The rationale for selecting these control variables was discussed in detail in our previous report (Trenholm 2000).

## B. FORMING THE SAMPLE FOR THE ANALYSIS OF IMPACTS

A number of records were dropped from the samples used in the analysis of demonstration impacts on service use. Below, we summarize the reasons for these exclusions.

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[4]In our previous report, Trenholm (2000) included only those Part B claims that were subject to prospective payment in the outcome measure. These claims were for patients with no Part A coverage who received home health care that would otherwise have been covered by Part A. In the current report, we included all Part B home health claims. Due to a change in HCFA regulations, all home health services beyond 100 visits and all visits that do not follow a hospital stay are paid under Medicare Part B. We would have excluded a significant proportion of home health services if these visits were not included in our analysis.

TABLE II.1

OUTCOME VARIABLES DESCRIBING MEDICARE SERVICE USE AFTER ADMISSION TO A DEMONSTRATION HOME HEALTH AGENCY

| |
|---|
| **Service Use**[a] |
| Total Number of Visits |
| Probability of a Visit (Each Type) |
| Number of Skilled Nursing Visits |
| Number of Home Health Aide Visits |
| Number of Physical Therapy Visits |
| Number of Occupational Therapy Visits |
| Number of Speech Therapy Visits |
| Number of Medical Social Worker Visits |
| Number of Episodes Completed |
| **Timing and Duration of Services**[b] |
| Days Until Last Visit |
| Probability of Last Visit, for Each Month After Admission |

[a]Outcomes variables for impact analysis. Measured from admission through 120 days.

[b]Additional outcome variables for long-stay analysis. Measured over a maximum of 720 days. Patients remaining in care past 720 days are assigned a censored value of 721 days.

TABLE II.2

CONTROL VARIABLES FOR MAIN MULTIVARIATE ANALYSIS, BY SOURCE

| Patient Level | | Agency Level | | Area Level |
|---|---|---|---|---|
| Patient Characteristics at Admission[a] | Medicare Service Use Prior to Admission[b] | Base-Quarter Patient Service Use[c] | Agency Characteristics[d] | Area Characteristics[e] |
| Age | Length of preadmission inpatient stay (if any) | Agency practice-pattern index | Chain member | Physicians per 10,000 (1994) |
| Race | | | Hospital-based | |
| Gender | Whether in skilled nursing facility within 14 days before admission | | Proprietary | Nursing home beds per 100 elderly residents (1991) |
| Original reason for entitlement | Number of home health visits in six months prior to admission | | Small agency (<30,000 visits in base year) | Hospital occupancy rate (1993) |
| Whether has cancer | | | State | |
| Whether has diabetes | | | Rural | |
| Whether has decubiti | | | | |
| Whether needs complex wound care | | | | |
| Whether has limitations in activities of daily living | | | | |
| Whether admitted to home health from hospital | | | | |
| Whether has Medicaid | | | | |
| Date of admission | | | | |

[a]From Medicare Enrollment Database; UB-92 remarks.

[b]From Medicare standard analytic files.

[c]From Abt base-quarter case-mix file.

[d]From base-year cost reports and Abt enrollment file.

[e]From Area Resource File.

## 1. Excluding Agencies

We dropped all of an agency's data from the analysis sample, for one of three reasons. Data were excluded if the agency: (1) dropped out of the demonstration before its completion,[5] (2) stopped admitting or providing visits to Medicare patients, or (3) had significant delays in billing during the first demonstration year, making it likely that their service use data were missing.[6,7] A total of 17 agencies (12 treatment and 5 control) were excluded from the analysis (Table II.3). After these agency-level exclusions, we were left with 74 agencies, 36 in the treatment group and 38 in the control group. Data on a total of 18,011 admissions from the 17 excluded agencies were dropped from the analysis (Table II.4).

## 2. Excluding Individual Admissions

We excluded individual patient records, for several reasons. We dropped 9,327 patient records because HCFA's enrollment files showed that they had been enrolled in Medicare HMOs on or after the episode start date. It would have been inappropriate to include these records because the services provided to managed care patients are not subject to prospective payment (or to traditional cost

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[5]One additional agency that dropped out two months before completion of the demonstration was retained. This agency had a sufficient number of visits and a complete record of billing data to allow us to retain information on their admissions and visits for the first eight months of their third year of the demonstration.

[6]Of the nine agencies that did not bill for a substantial share of their services, eight were in the treatment group. Furthermore, part of the problem was related to the payment system, as some of the PPS agencies either failed to submit (or resubmit) bills for services provided in the first 120 days because they had been reimbursed through the episode payment. Trenholm (2000) conducted sensitivity analyses to determine whether excluding these agencies accounted for the large impacts reported. These analyses found no evidence that the reported large impacts of prospective payment were due to the exclusion of these agencies.

[7]One of the agencies that was diagnosed as having billing problems in the previous analysis (Trenholm 2000) subsequently corrected these problems and submitted a complete claims file for the three demonstration years. For the current report, this agency was put back in the analysis file.

TABLE II.3

AGENCY DATA EXCLUDED FROM ANALYSIS SAMPLE, BY TREATMENT STATUS

| | Agency Status | | Percentage of Original Total |
|---|---|---|---|
| | Control | Treatment | Control |
| Main File | 43 | 48 | 100.0 |
| Dropped Out of Demonstration | 3 | 3 | 6.6 |
| Stopped Serving Medicare Patients | 1 | 2 | 3.3 |
| Bad Billing Data | 1 | 7 | 8.8 |
| **Sample Available for Analysis** | **38** | **36** | **81.3** |

TABLE II.4

PATIENT ADMISSIONS RECORDS EXCLUDED FROM
ANALYSIS SAMPLE, BY TREATMENT STATUS

| | Year 1 | | Year 2 | | Year 3 | | Total | |
|---|---|---|---|---|---|---|---|---|
| | Control | Treatment | Control | Treatment | Control | Treatment | Control | Treatment |
| **Main File**[a] | 32,995 | 36,968 | 27,771 | 32,105 | 21,456 | 23,404 | 82,222 | 92,477 |
| **Agency-Level Exclusions** | | | | | | | | |
| Dropped Out of Demonstration | 3,506 | 688 | 662 | 231 | 33 | 10 | 4,201 | 929 |
| Missing Bill Data | 851 | 4,739 | 698 | 3,724 | 295 | 2,574 | 1,844 | 11,037 |
| **Patient-Level Exclusions** | | | | | | | | |
| Enrolled in HMO[b] | 2,069 | 2,327 | 1,838 | 1,542 | 768 | 783 | 4,675 | 4,652 |
| Has Medicare as Second Payer | 878 | 907 | 862 | 872 | 679 | 552 | 2,419 | 2,331 |
| Missing Data | 980 | 509 | 727 | 705 | 4,647 | 3,128 | 6,350 | 4,341 |
| Prior Demonstration Admission | 848 | 1,270 | 3,710 | 4,672 | 3,393 | 3,906 | 7,951 | 9,848 |
| **Sample Available for Analysis** | 23,863 | 26,528 | 19,274 | 20,359 | 11,641 | 12,451 | 54,782 | 59,339 |

[a]All demonstration admissions through August 31, 1998.

[b]Health Maintenance Organization.

reimbursement). We dropped 4,750 patient records because Medicare was a secondary payer for the patient on or after the episode start date. As in the case of managed care patients, the services provided to these patients are not subject to per-episode payment; thus, agencies' behavior would be largely independent of the demonstration incentives.

We dropped 10,696 patient records because they had missing data. In the case of 3,435 of these records, the data fields of their UB-92 forms did not contain complete information on patient characteristics. We had to exclude data on another 2,323 patient admissions that occurred in the last four months of an agency's third demonstration year because we would not be able to obtain service use data on the entire "at-risk" period of the episode. In addition, these admissions in the "phase-out" period of the demonstration were not subject to per-episode reimbursement, as were admissions throughout the rest of the demonstration. The individual records excluded from the analyses are detailed in Table II.4.

### 3. Excluding Subsequent Patient Episodes

In constructing the analysis file, we were concerned that, if we included subsequent episodes beyond the first episode of care, we would not be able to use one of our most important control variables: home health use in the preceding six months. Prior home health use would become correlated with the treatment status of the agency to which the patient was admitted.[8] The difficulty with excluding subsequent episodes is that the patient populations from the three demonstration years become less similar, and therefore, the comparison of impacts across these years is more difficult. We chose to exclude a patient's subsequent episodes occurring in the same demonstration

[8]If we included subsequent episodes, then patients who were previously admitted to treatment group agencies would have fewer home health visits in the prior six-month period than control group agency patients, due to the effects of prospective payment on service provision. Since the downward bias would only be for episodes in the treatment group, including it in the regression would bias our impact estimates.

year or any subsequent year. This resulted in the exclusion of 17,799 records from the analysis sample. The sensitivity of our estimates to the exclusion of these subsequent episodes is discussed in detail in Chapter III.

### 4. Analysis Sample

After dropping all these records from the main file, we arrived at an analysis sample that contained 114,121 unique patient admissions (54,782 in the control group and 59,339 in the treatment group). The sample available for analysis decreases over the three years of the demonstration. There are a total of 50,391 admission records in Year 1, 39,633 in Year 2, and 24,092 in Year 3. There are two reasons for this decrease: (1) data from subsequent admissions were excluded, and (2) data from admissions in the phase-out period were excluded. It should be noted that even before we began to exclude data at either the agency or the patient level, the raw data file also showed a large decrease in admissions over the course of the demonstration.[9]

## C. SUMMARY STATISTICS FOR THE MAIN CONTROL VARIABLES IN THE IMPACT ANALYSIS

The regression models used to estimate demonstration impacts contain a number of independent variables. Despite random assignment of agencies to the treatment and control groups, unequal study groups may have been caused by the need to drop several agencies from the analysis, or simply from the overall small number of agencies in the sample. The regression models allow us to control for any preexisting differences between treatment and control agencies.

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[9]Before exclusions were made, the main data file contained 69,963 admissions for Year 1, 59,876 admissions for Year 2, and 44,860 admissions in Year 3.

## 1. Characteristics of the Sample

As shown in Table II.2, the control variables used in our regression models include patient-, agency-, and area-level characteristics. In Table II.5, we display the sample means for patient-level control variables for patient admissions in the first, second, and third demonstration years.[10]

The means were constructed using sample weights that give each agency equal importance in the calculations. The means are given for each year, as well as for the treatment and control groups separately, in order to determine if there were any significant differences in the patient populations between the groups.[11]

We use SUDAAN to account for the design effects associated with use of the sample weights. This correction is reflected in the significance levels for the treatment and control group means. We did not account for design effects due to clustering because we are only interested in differences within the sample, not the population of all agencies. Although we did not account for the effects of clustering in the construction of control variables, for the main impact analysis we will need to be concerned with design effects due to clustering.

### a. Patient Demographics

In each of the three demonstration years, the majority of the patients in the sample were female (64 percent) and white (81 percent). The largest percentage of the sample population, about 45 percent, was between 75 and 84 years old. In each of the three years, the mean age of the patients

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[10]As discussed previously, the data set for Year 3 contains admissions for the first eight months of that year.

[11]With the large sample available, sufficient statistical power exists to detect very small differences between the treatment and control groups. We therefore expected that many differences in the explanatory variables between the treatment and control group agencies would be statistically significant even when the magnitude of the difference was not materially important.

TABLE II.5

WEIGHTED MEANS FOR PATIENT-LEVEL CONTROL VARIABLES, BY TREATMENT STATUS, AND TESTS FOR DIFFERENCES IN TREATMENT AND CONTROL GROUP MEANS

| | Year 1 | | Year 2 | | Year 3 | |
|---|---|---|---|---|---|---|
| Explanatory Variable | Treatment Group | Control Group | Treatment Group | Control Group | Treatment Group | Control Group |
| **Demographic Measures (Percentage)** | | | | | | |
| Age Was Original Reason for Medicare Eligibility | 84.5 | 81.6*** | 84.0 | 80.9*** | 83.6 | 81.8** |
| Age: <65 Years | 7.2 | 8.6*** | 8.3 | 9.5** | 8.2 | 9.3* |
| Age: 65 to 74 Years | 27.9 | 29.8*** | 27.1 | 29.1*** | 27.2 | 29.4** |
| Age: 75 to 84 Years | 40.6 | 39.9*** | 41.3 | 40.2 | 40.2 | 38.5 |
| Age: >84 Years | 24.1 | 21.6 | 23.2 | 21.2*** | 24.4 | 22.7* |
| Race/Ethnicity: White | 81.9 | 81.6 | 82.2 | 79.8*** | 81.5 | 79.5** |
| Female | 64.4 | 64.0 | 64.2 | 63.3 | 63.1 | 65.4** |
| Enrolled in Medicaid | 25.7 | 24.2** | 26.1 | 24.7* | 25.8 | 24.7 |
| **Medical Conditions and Care (Percentage)** | | | | | | |
| Cancer | 13.7 | 13.4 | 12.7 | 13.1 | 12.7 | 10.4*** |
| Diabetes | 20.9 | 21.4 | 19.3 | 20.6* | 19.6 | 21.3* |
| Cerebrovascular Accident (Stroke) | 14.5 | 14.4 | 13.1 | 14.9*** | 12.1 | 15.4*** |
| Decubitus Ulcer: Stage III or IV | 4.6 | 3.7*** | 4.0 | 4.6 | 4.1 | 5.3** |
| Needs Complex Wound Care | 6.8 | 6.7 | 7.4 | 7.3 | 8.8 | 8.1 |
| Any of the Medical Conditions | 46.4 | 45.9 | 43.8 | 46.2*** | 44.4 | 45.8 |
| Bathing | 71.7 | 73.3*** | 70.8 | 70.6 | 69.3 | 72.4*** |
| Eating | 26.6 | 29.4*** | 24.1 | 26.0** | 23.3 | 25.3** |
| Dressing | 60.3 | 64.7*** | 60.3 | 62.3** | 58.3 | 61.3*** |
| Toileting | 37.2 | 40.7*** | 33.8 | 35.6** | 33.8 | 36.3** |
| Transferring | 50.0 | 52.4*** | 50.2 | 51.1 | 50.7 | 51.5 |
| Any ADL[a] | 77.3 | 78.5** | 77.8 | 77.4 | 77.8 | 78.7 |
| **Patient Prior Service Use Measures** | | | | | | |
| Admitted to Home Health from Hospital (Percentage) | 35.3 | 39.1*** | 39.3 | 40.1 | 41.5 | 39.9 |
| Length of Hospital Stay Ending Within 14 Days Before Home Health Admission (Number of Days) | 3.7 | 4.21*** | 3.7 | 4.01*** | 4.1 | 4.14 |
| Had SNF Stay Ending Within 14 Days Before Admission (Percentage) | 16.6 | 14.5*** | 18.3 | 15.3*** | 19.6 | 16.3*** |
| Total Part A (Plus Part B Inpatient/SNF/Home Health/Hospice) Reimbursement in Six Months Before Admission (Dollars) | 11,193 | 10,747* | 11,709 | 10,974*** | 12,795 | 11,729*** |

TABLE II.5 *(continued)*

| Explanatory Variable | Year 1 | | Year 2 | | Year 3 | |
|---|---|---|---|---|---|---|
| | Treatment Group | Control Group | Treatment Group | Control Group | Treatment Group | Control Group |
| Part A Home Health Visits in Six Months Before Admission (Number) | 11.8 | 12.4 | 11.4 | 10.9 | 14.7 | 15.8 |
| Had Medicare for Less than Six Months Before Admission | 1.3 | 1.3 | 1.7 | 1.7 | 1.5 | 1.3 |
| Enrolled in HMO in Six Months Before Admission | 0.9 | 0.8 | 1.0 | 1.0 | 0.9 | 1.1 |
| Had Medicare as Secondary Payer in Six Months Before Admission | 0.3 | 0.3 | 0.3 | 0.3 | 0.3 | 0.3 |
| Admission Date: 5/30/95 - 9/30/95 | 4.5 | 7.5*** | - | - | - | - |
| Admission Date: 10/1/95 - 12/31/95 | 10.3 | 12.1*** | - | - | - | - |
| Admission Date: 1/1/96 - 3/31/96 | 27.7 | 29.0* | - | - | - | - |
| Admission Date: 4/1/96 - 6/30/96 | 25.1 | 23.4*** | 0.2 | 0.2 | - | - |
| Admission Date: 7/1/96 - 9/30/96 | 18.6 | 16.8*** | 3.9 | 6.5*** | - | - |
| Admission Date: 10/1/96 - 12/31/96 | 13.7 | 11.1*** | 10.1 | 13.1*** | - | - |
| Admission Date: 1/1/97 - 3/31/97 | - | - | 27.0 | 27.6 | - | - |
| Admission Date: 4/1/97 - 6/30/97 | - | - | 25.6 | 23.9** | 0.3 | 0.5 |
| Admission Date: 7/1/97 - 9/30/97 | - | - | 19.5 | 17.4*** | 5.0 | 10.7*** |
| Admission Date: 10/1/97 - 12/31/97 | - | - | 13.7 | 11.1*** | 13.8 | 17.4*** |
| Admission Date: 1/1/98 - 3/31/98 | - | - | - | - | 37.1 | 34.2*** |
| Admission Date: 4/1/98 - 6/30/98 | - | - | - | - | 29.6 | 23.9*** |
| Admission Date: 7/1/98 - 9/30/98 | - | - | - | - | 14.2 | 13.3 |
| **Sample Size: Patient Episodes** | 26,528 | 23,863 | 20,359 | 19,274 | 12,451 | 11,641 |

[a]Aetivities of Daily Living.

*Difference in means is signifieantly different from zero at the .10 level, two-tailed test.
**Difference in means is signifieantly different from zero at the .05 level, two-tailed test.
***Difference in means is signifieantly different from zero at the .01 level, two-tailed test.

in the sample was slightly older among treatment agencies. For approximately 83 percent of the sample, age was the original reason for Medicare eligibility. Approximately 25 percent of the sample were also enrolled in the Medicaid program. Patients in treatment agencies were slightly more likely to have age as their original reason for entitlement, be white and female, and be dually eligible for Medicaid.

**b. Medical Conditions and Care**

At the time of admission, approximately 13 percent of patients in the sample had cancer, 20 percent had diabetes, and 14 percent were recovering from a stroke. A smaller number had a Stage III or IV decubitus ulcer (4 percent), or needed skilled nursing for complex wound care (seven percent). There was little difference in the prevalence of these conditions between patients in treatment and in control group agencies.

**c. Activities of Daily Living**

We were able to obtain data on the ability of patients to perform five activities of daily living (ADLs). Approximately 78 percent of the sample population had difficulty performing at least one ADL. The most frequently recorded deficit was in bathing, where approximately 71 percent of patients needed assistance. About 61 percent had difficulty with dressing, 51 percent with transferring from bed or chair, 35 percent with toileting, and 25 percent with eating.

On the whole, patients in treatment group agencies had fewer limitations in performing activities of daily living than did control group patients. Although these differences were significant, the actual magnitude of the differences was small, from about two to seven percent of the control group mean.

### d. Prior Service Use Measures

Approximately 39 percent of both treatment and control group agency patients were admitted to home health directly from a hospital. For treatment group agency patients, this percentage increased from 35.3 percent in the first demonstration year to 41.5 percent in the third year of the demonstration. The average length of stay for prior hospitalization was approximately four days for both treatment and control group agency patients. About 17 percent of all patients had been in a skilled nursing facility (SNF) in the 14 days prior to their admission to home health. Treatment group patients were significantly more likely than control group patients to have an SNF stay in each of the three demonstration years. Treatment group agency patients also had significantly higher Medicare Part A (plus Medicare Part B inpatient, SNF, home health, and hospice) costs in the six months before their index home health admission. There was no difference between treatment and control agencies in the number of home health visits their patients had during the six months before their current home health admission.

### e. Agency-Level Characteristics

The agency- and area-level control variable means are shown in Table II.6. Overall, 48.6 percent of the agencies in our sample were for-profit, and 35.1 percent were part of a chain. About 23 percent were considered small agencies, providing fewer than 30,000 patient visits in the demonstration baseline year. Only 12.2 percent of agencies were hospital-based.

There were significant differences in the practice patterns of treatment and control group agencies. Based on a case-mix-adjusted index of agencies' average visits per episode during the baseline year, the mean practice pattern is measured as 1.12 in control group agencies and 0.96 in treatment group agencies. This difference, about 14 percent of the control group mean, essentially indicates that the average treatment group agency provided about 14 percent fewer visits to a typical

TABLE II.6

WEIGHTED MEANS FOR AGENCY-LEVEL AND AREA-LEVEL CONTROL VARIABLES, BY TREATMENT STATUS, AND TESTS FOR DIFFERENCES IN TREATMENT AND CONTROL GROUP MEANS

| Explanatory Variable | Total | Treatment Group | Control Group |
|---|---|---|---|
| **Agency Characteristics Measures (Percentage)** | | | |
| Proprietary | 48.6 | 44.4 | 52.6*** |
| Hospital-Based | 12.2 | 8.3 | 15.8*** |
| Chain | 35.1 | 41.7 | 28.9*** |
| Small Agency (<30,000 Visits in Base Year) | 22.9 | 27.8 | 18.4*** |
| Agency Practice Pattern: Index of Average Visits | 1.04 | 0.96 | 1.12*** |
| **Area Characteristics Measures** | | | |
| California (Percentage) | 24.3 | 25.0 | 23.7*** |
| Florida (Percentage) | 6.7 | 8.3 | 5.3*** |
| Illinois (Percentage) | 17.6 | 11.1 | 23.7*** |
| Massachusetts (Percentage) | 14.8 | 22.2 | 7.9*** |
| Texas (Percentage) | 36.5 | 33.3 | 39.4*** |
| Urban (Percentage) | 86.5 | 88.9 | 84.2*** |
| Physician per 10,000 Persons (Number; 1994) | 22.1 | 22.9 | 21.3*** |
| Nursing Home Beds per 100 Person >65 Years (Number; 1991) | 5.2 | 5.0 | 5.3*** |
| Hospital Occupancy Rate (Percentage; 1993) | 62.3 | 63.9 | 60.8*** |

[a]An index of the case-mix adjusted average visits received by an agency's patients during the first 120 days of base-year quarter episodes, relative to the average across all agencies.

***Difference in means is significantly different from zero at the .01 level, two-tailed test.

patient before the demonstration even started. Because agencies' practice patterns almost certainly carry over from one year to the next, it is critical to control for these preexisting differences in practice patterns in the regression models.

Treatment group agencies were significantly more likely to be larger, nonprofit, nonhospital-based, and part of a chain. These differences were not only statistically significant, but also materially large.

### f. Area Characteristics

The majority of agencies (86.5 percent) were located in urban areas, with the largest number from Texas (36.5 percent), followed by California with 24.3 percent and Illinois with 17.6 percent. Massachusetts and Florida had a smaller number of agencies in the demonstration, with 14.8 percent and 6.7 percent, respectively.

The relative proportions of treatment and control agencies differed significantly in all five states. Treatment agencies were overrepresented in California, Florida, and Massachusetts and underrepresented in Illinois and Texas. Because the utilization of home care services varies widely across these states, it is important that the regression models control for these treatment-control differences.

Treatment agencies also were significantly more likely to be in areas that are (1) more urban, (2) have higher hospital occupancy rates, and (3) have more physicians per 10,000 residents, although these differences were not large. Treatment agencies were less likely to be found in areas with a greater number of nursing home beds per 100 people over age 65.

### 2. Implications for the Analysis

Although the preexisting treatment-control differences in patient characteristics are minimal, there are several large differences in agency and area characteristics.[12] These differences underscore the importance of using regression models to estimate program impacts, since these models control for effects on service use that might otherwise be falsely attributed to prospective payment. For example, the far lower service provision by treatment agencies prior to the demonstration (as measured by the index of practice patterns) suggests that a simple comparison of mean visits under the demonstration might substantially overstate the program impact. In Section II.E, we describe the regression models used to isolate the impact of the method of payment and address other issues involved in accurately estimating program effects.

## D. UNIT OF ANALYSIS FOR MEASURING DEMONSTRATION IMPACTS

We measure impacts on service use during the first 120 days after admission, or the so-called "at-risk" period, during which all service use falls under the episodic payment. Although this time period is considerably shorter than the 365-day ("patient-year") period used to examine service use in Trenholm (2000), it allows us to explore impacts taking place among patients admitted over the course of the demonstration. Moreover, given the general significance and stability of the impacts found by Trenholm (2000), this time period should be sufficient to characterize changes in impacts taking place throughout the demonstration.

As in Trenholm (2000), our analysis focuses only on the first admission for a patient taking place during the demonstration. By doing so, our impact estimates are unaffected by the possible

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[12]Some of the agency-level differences between the treatment and control groups can be attributed to the exclusion of 17 agencies' data from the analysis sample (discussed previously). However, the large differences in practice patterns and state of operation would have remained even with all 91 agencies in the sample.

correlation between treatment status and subsequent episode starts, and we avoid double-counting patients. While this approach will lead us to "miss" some amount of care that falls under the episodic payment, the amount is small, as there will be relatively few subsequent episodes.

## E. STATISTICAL TECHNIQUES FOR MEASURING DEMONSTRATION IMPACTS

We use regression models to estimate demonstration impacts, because they control for preexisting differences between treatment and control agencies that may exist despite random assignment and because they improve statistical precision. For our estimation of impacts through the three years of the demonstration, we use OLS regression when the dependent variable is continuous and logistic regression when the dependent variable is binary.

We also conduct several sensitivity analyses to examine the robustness of our impact estimates to such factors as data censoring, skewness, and the use of sample weights. Details on the methods used in these sensitivity tests are available in Trenholm (2000). All estimates from these regression models have standard errors that take into account the effects of sample clustering and weighting.

Throughout our tables of results, shown in Chapter III, alongside the estimated impact, we present the means of each outcome variable for the treatment group and the control group as points of reference. The control group mean is unadjusted and reflects a reasonable estimate of the mean value expected to occur in the absence of the demonstration. The treatment group mean is regression-adjusted by setting it equal to this control group mean, minus the (regression-based) impact estimate.

### 1. Estimating Impacts Between Demonstration Years

To examine whether the impact of the demonstration changes with agencies' length of participation, we estimate a regression model similar to the one detailed in Trenholm (2000). The

one difference is that the specification includes an additional dummy variable for the demonstration year in which the admission took place, as well as an interaction term between the agency's treatment status and the demonstration year. The full model is thus given by:

$$(1)\quad Y = \alpha + X\beta + \delta T + \lambda_1 Year2 + \lambda_2 Year3 + \gamma_1(T*Year2) + \gamma_2(T*Year3) + \varepsilon$$

where:

$Y =$ a continuous outcome variable, measured after a patient's admission to a demonstration home health agency

$Year\ 2 =$ a variable that equals one if the admission took place in *Year2* and zero otherwise

$Year\ 3 =$ a variable that equals one if the admission took place in *Year3* and zero otherwise

$X =$ the set of control variables

$T =$ a binary variable for treatment status that equals one for admissions to treatment agencies and a zero for admissions to control agencies[13]

$\lambda_1 =$ the coefficient on the variable *Year2*

$\lambda_2 =$ the coefficient on the variable *Year3*

$\gamma_1 =$ a coefficient on the interaction term between the variables *Year2* and *T* (where this interaction term equals one for admissions in demonstration *Year2* to treatment agencies and zero otherwise)

$\gamma_2 =$ a coefficient on the interaction term between the variables *Year3* and *T* (where this interaction term equals one for admissions in demonstration *Year3* to treatment agencies and zero otherwise)

$\alpha =$ the intercept term

$\beta =$ the vector of regression coefficients on the control variables

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[13] The omitted (reference) binary variable equals one for admissions to control agencies and zero for admissions to treatment agencies.

$\delta$ = a regression coefficient on the variable for treatment status

$\varepsilon$ = a random disturbance term assumed to have a mean of zero (conditional on the regressors in the model) that reflects all the unobserved factors affecting the outcome variable

The impact estimates available from this model are as follows. The estimated impact of prospective payment in the first demonstration year is given by $\delta$. The estimated impact in the second demonstration year is the sum of the coefficients $\delta$ and $\gamma_1$. The estimated impact in the third demonstration year is the sum of coefficients $\delta$ and $\gamma_2$. The difference in the Year 2 and Year 1 impacts, $(\gamma_1)$, measures the change in the effect of prospective payment from the first to the second years of the demonstration. The difference in the Year 3 and Year 2 impacts, $(\gamma_2 - \gamma_1)$, measures the change from the second to the third year of the demonstration.

## 2. Estimating Subgroup Impacts

The most significant subgroup difference found in Trenholm (2000) was that agencies with "high-use" practice patterns were able to achieve the greatest reductions in service use.[14] We will repeat this agency subgroup analysis to determine whether impacts, if they continue, are significant across most or all of the agency subgroups. In addition, we will examine impacts for two other agency subgroups, size and for-profit status, since differences for these groups were large but are closely linked to practice patterns.[15]

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[14]Practice patterns of each agency were defined from a case-mix-adjusted index of agencies' service provision in the base year. High-use agencies have an index value above the median value; low-use agencies are at or below the median value. See Section C for additional information.

[15]Other agency subgroups that we investigated in Trenholm (2000) were defined by auspice (hospital-based or freestanding) and whether the agency had costs above or below their limits in the year prior to the demonstration. While some significant differences were found for these subgroups, the very small samples of hospital-based agencies and agencies above the cost limits offer very limited statistical power with which to explore impacts over time for these subgroups. Trenholm (continued...)

Our model for examining subgroup impacts by practice patterns is as follows:

$$(2)\quad Y = \alpha + X\beta + \delta T + \varphi H + \lambda_1 Year2 + \lambda_2 Year3 + \gamma_1(T*Year2) + \gamma_2(T*Year3) +$$

$$\theta\,(H*T) + \beta_1\,(H*Year2) + \beta_2\,(H*Year3) + \alpha_1\,(H*T*Year2) + \alpha_2\,(H*T*Year3) + \varepsilon$$

where:

$Y =$ a continuous outcome variable, measured after a patient's admission to a demonstration home health agency

$Year2 =$ a variable that equals one if the admission took place in *Year2* and zero otherwise

$Year3 =$ a variable that equals one if the admission took place in *Year3* and zero otherwise

$X =$ a set of control variables (excludes the variables that controls for practice patterns)

$T =$ a binary variable for treatment status that equals one for admissions to treatment agencies and a zero for admissions to control agencies[16]

$H =$ a binary variable for agency practice pattern that equals one for admissions to high-use agencies and a zero for admissions to low-use agencies

$\delta =$ a coefficient on the variable for treatment status

$\varphi =$ a coefficient on the variable $H$

$\lambda_1 =$ a coefficient on the variable *Year2*

$\lambda_2 =$ a coefficient on the variable *Year3*

$\gamma_1 =$ a coefficient on the interaction term between the variables *Year2* and $T$

$\gamma_2 =$ a coefficient on the interaction term between the variables *Year3* and $T$

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[15](...continued)
(2000) also reported findings for selected patient subgroups. However, across these patient subgroups, impacts were similar as a proportion, and any statistical differences were only marginally significant.

[16]The omitted (reference) binary variable equals one for admissions to control agencies and zero for admissions to treatment agencies.

$\theta =$ a coefficient on the interaction term between the variables *H* and *T*

$\beta_1 =$ a coefficient on the interaction term between the variables *H* and *Year2*

$\beta_2 =$ a coefficient on the interaction term between the variables *H* and *Year3*

$\alpha_1 =$ a coefficient on the interaction term between the variables *H, Year2*, and *T*

$\alpha_2 =$ a coefficient on the interaction term between the variables *H, Year3*, and *T*

$\alpha =$ the intercept term

$\beta =$ the vector of regression coefficients on the control variables

$\varepsilon =$ a random disturbance term assumed to have a mean of zero (conditional on the regressors in the model) that reflects all the unobserved factors affecting the outcome variable

The impacts for subgroups from this model are as follows. The high-use/low-use subgroup difference in the estimated impact of prospective payment for Year 1 is ($-\theta$). In Year 2, the subgroup difference is ($-\theta - \alpha_1$). In Year 3, the subgroup difference is ($-\theta - \alpha_2$). To assess whether the differences in impacts between the high-use and low-use subgroups are significant, we examine the statistical significance of these coefficients.[17]

In order to investigate impacts by size and for-profit status, we could estimate a similar model to equation (2). However, in Trenholm (2000), impacts for these subgroups were found to be closely linked to practice patterns. Therefore, we prefer to estimate impacts for these subgroups controlling for subgroup differences by practice patterns. To do this, we augment equation (2) with an additional set of interaction terms reflecting whether the agency is small/for-profit. Operationally, this amounts to repeating the five interaction terms shown on the second row of equation (2) and

[17]Larger impacts were found for-profit agencies and small agencies than for their counterparts (small and low-use agencies, respectively). However, these differences were largely explained by the greater likelihood that these agency types had high-use practice patterns.

replacing the dummy variable for high-use practice patterns ($H$) with a dummy variable for for-profit status or small size, as appropriate.

### 3. Hypothesis Tests for the Impact Estimates

For each outcome, a two-tailed $t$-statistic tests the null hypothesis that there is no difference between the regression-adjusted population means for treatment and control agencies.[18] The associated $p$-value, which indicates the probability of obtaining a sample estimate of the observed magnitude if the null hypothesis was true, is used to determine whether the demonstration had a measurable impact. The $p$-value is based on estimated standard errors that account for the clustering of patients within agencies and the use of sample weights. A $p$-value of less than .10 indicates rejection of the null hypothesis and provides significant statistical evidence that a demonstration impact probably exists. At this $p$-value, however, approximately 10 percent of independent tests will show, simply by chance, a statistically significant treatment-control difference when there is no true program effect (known as Type I error). Therefore, in assessing whether a statistically significant treatment-control difference (especially one with a $p$-value between .05 and .10) should be interpreted as a true program impact, we consider whether the sign and magnitude of the predicted effect are consistent with those for related outcomes.

## F. DESCRIBING LONG-STAY PATIENTS

For our second analysis in this report, we characterize the group of patients that have very long stays in home health.

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[18]Two-tailed tests are used throughout our analysis to avoid confusion and to flag estimates of the "wrong" expected sign that are large enough to be statistically significant. For impacts with the "correct" expected sign, a two-tailed test is less likely than a one-tailed test to reject the hypothesis of no demonstration effect (all else equal).

## 1. Forming the Sample for the Long-Stay Analysis

We defined long-stay patients as those whose length of stay in home care is greater than 365 days since the time of their admission. In constructing the sampling frame for these patients, we faced a trade-off between including more patients and following patients for a longer period of time. We chose the latter option, believing that it is more important to follow these patients as long as possible, to get a better understanding of their long-term service use. Therefore, we included all patients admitted to a demonstration agency between January 1, 1996 and December 31, 1996, with an admission duration of at least 365 days. Although we could have included all long-stay patients from the beginning of the demonstration in May 1995, we did not want to bias the sample with the long-stay patients from control group agencies. Many of the control group agencies started the demonstration before treatment group agencies did. If we included control group data from the start of the demonstration, we would create unmatched time periods for the analysis. To the extent that the early months of the demonstration reflect different environmental and policy conditions external to the demonstration, the behavior of control group agencies may have been different in this time period.

Given that these long-stay patients could have been admitted as late as December 31, 1996, a maximum two-year follow-up period is possible. We followed these patients for two years from their date of admission or to their date of discharge, whichever occurs first.

To form the sample for this analysis, we started with the same basic sample as previously described for the impacts analysis. We identified patient episodes in the same way--that is, from admission to discharge or a 45-day gap in care. We then used the same criteria to exclude records at both the agency and the patient level. The result was a sample of 3,311 long-stay patients who

were admitted to home health care between January 1 and December 31, 1996 and who had a length of stay of at least one year.

Because the majority of care provided to these patients is beyond the 120-day at-risk period for prospective payment, we did not attempt to measure demonstration impacts for this group. Similarly, because we followed these patients until their point of discharge, the unit of analysis here is the "natural" episode rather than the "at-risk" period that was used in the impacts analysis. We defined the "natural" episode as the time from admission to discharge, or a 45-day gap in care.

### 2. Descriptive Methods for Long-Stay Analysis

For our description of long-stay patients, we compare demographic and clinical characteristics of patients on admission to either treatment or control group agencies. We use *t*-tests to assess the differences between the treatment and control groups.

# III. IMPACT OF PROSPECTIVE PAYMENT ON SERVICE USE: TRENDS OVER THE FULL DEMONSTRATION

To assess the potential impact of a home health prospective payment system on the use of home health services, we must examine changes in the use of services over the full three years of the demonstration. In our previous report (Trenholm 2000), we found that treatment agencies were able to make sharp cuts in the number of home health visits they provided. We saw this large impact, both in Year 1 of the demonstration and in the available data for Year 2. In this chapter, we compare impacts on key outcomes in all three demonstration years, to determine how these impacts evolved over the entire demonstration.

## A. PRIOR FINDINGS

Trenholm (2000) highlighted four major results from the first two years of the demonstration. First, reductions in service use occurred both during and after the at-risk period, and they were of a similar proportion for each type of home health visit. For example, within the *at-risk period*, prospectively paid agencies reduced the total visits per patient from about 45 to 37, a decline of 17 percent. Skilled nursing visits accounted for about half of the decline, falling an average of 4 visits per patient, or 19 percent. In addition, in the *year* following a patient's admission, prospective payment reduced the average number of visits by 24 percent compared to the average under cost-based reimbursement.

Second, this report showed that prospectively paid agencies achieved decreased service use largely by shortening the overall length of service. Based on episodes observed over a maximum period of 480 days, the average episode length fell from 131 days to 98 days--a drop of more than

one month (Trenholm 2000). This decline resulted from broad-based reductions in episode lengths for all types of patients.

Third, impacts on service use varied by agency subgroup. Agencies providing a relatively large number of visits per episode prior to the demonstration cut more visits per patient than did relatively "low-use" agencies (about 24 visits per patient in the year after admission for high-use agencies, compared to 12 visits for low-use agencies). Actually, these declines were quite similar in percentage terms (about 23 percent and 26 percent, respectively). Other subgroup differences (for-profit/nonprofit, small/large, freestanding/hospital-based) were also evident but appear to be associated with high-use practice patterns. For example, for-profit agencies made larger cuts in visits than nonprofits, but the difference was due entirely to a higher proportion of for-profit agencies with high-use practice patterns prior to the demonstration. Patient characteristics (for example, the ability to take oral medications independently) did not have a major influence on the magnitude of demonstration impacts.

Finally, the available data from Year 2 suggested that the impacts of prospective payment remained stable over time.[1] However, agencies' behavior did change significantly. Specifically, both treatment and control agencies made large and significant reductions in visits and service length in Year 2 (compared to Year 1), which resulted in no change in the effect of prospective payment between the two years.

Based on these strong and sustained results from Year 1 and Year 2, we were unsure what to expect in the data from Year 3. For example, would agencies be able to continue to cut visits?

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[1]Trenholm (2000) examined impacts on the eight months of data available for Year 2.

Would service use plateau? Or would agencies feel that they had reduced visits too sharply, and perhaps begin to provide additional visits?

## B. EXPECTED TRENDS

In addition to the demonstration incentives, several environmental factors contributed to reduced service use among both treatment and control agencies (Cheh et al., forthcoming). The ongoing efforts of Operation Restore Trust (ORT) made physicians more wary of authorizing large numbers of home health visits for their patients. Provisions of BBA 1997 redefined the requirements for "part-time" and "intermittent" requirements under the Medicare home health benefit and eliminated coverage for venipuncuture when it is the only skilled service required. Furthermore, BBA 1997 introduced on Interim Payment System (IPS) for agencies not participating in the demonstration which provided agencies on incentive to reduce services. All of these factors may have helped reduce the average number of visits provided by home health agencies. Also, managed care continued to be a major influence in many areas, requiring agencies to provide quality home health care while restricting the number of visits provided. The administrative and clinical structures used by agencies to help them meet the requirements of their managed care contracts may have helped them operate more effectively under prospective payment. Either factor could contribute to continuing reduction in service use by agencies in the demonstration, and would thus influence treatment and control agencies alike.

Over time, we may see increased demonstration impacts because many of the initiatives used by treatment agencies to reduce visits took time to implement. For example, during our site visits, numerous agencies commented that demonstration requirements (such as changing billing procedures and completing quality assurance forms) took some time to adjust to and that some staff members were reluctant to change their patterns of care (Phillips and Thompson 1997). If these

factors significantly delayed the time it took agencies to introduce a structured approach to reducing visits, we might expect impacts to be even larger in the third demonstration year.

Alternatively, demonstration impacts may weaken over time, or they may go away entirely. Perhaps agencies felt that they had cut as much as they could, and were unable or unwilling to make further cuts. Moreover, they may have felt that they had cut too far, adversely affecting patient outcomes. Many demonstration agencies have a high percentage of Medicare patients; for this group of patients, agencies compete for referrals based largely on the quality of care they provide. Treatment group agencies may be reluctant to continue cutting visits if they think their referral sources might begin to doubt their ability and commitment to providing quality care. If this is the case, it may cause impacts to weaken.

Despite this mix of influences, we expect demonstration impacts to continue into Year 3, although we anticipate that the rate at which agencies cut visits may begin to slow. Both treatment and control agencies observed that agencies outside the demonstration had been forced by the Interim Payment System (IPS) to control costs. Although it is possible that the demonstration agencies believed they had cut as many visits as they could, they likely would continue trying to cut any unnecessary visits, in preparation for implementation of the actual PPS.

## C. OVERALL IMPACTS

As discussed in Chapter II, we have limited our analysis to the at-risk period (that is, the first 120 days) of the patient episode. Treatment group means have been regression-adjusted using logit or OLS models to control for preexisting differences in patients or agencies between treatment and control groups. All impacts reported in this chapter refer to this time point. We examine demonstration impacts on four measures of service use: (1) total number of visits provided, (2) number of visits by type, (3) probability of receiving certain types of visits, and (4) episode length.

## 1. Total Number of Visits Provided

The total number of visits provided by 120 days after admission by *all* demonstration agencies continued to decrease in Year 3; however impacts remained stable and may even have increased slightly (Table III.1). In Year 1, by the 120th day of admission, treatment agencies provided an average of 37.6 visits, whereas control agencies provided an average of 45.3. By Year 3, the total number of visits provided by 120 days dropped to 32.0 for treatment agencies. Control group agencies however, also reduced the average total number of visits they provided to 41.0. The behavior of control group agencies is particularly striking. Although they had not caught up with the treatment agencies, by Year 3, the controls had made fairly large reductions in the number of visits they provided ($p = 0.59$ for Year 1 versus Year 3). Thus, despite the large reductions in the number of visits provided by treatment group agencies, the overall impacts in each year were relatively constant.

The large reductions in visits between Year 1 and Year 3 for control group agencies makes one wonder if changes in patient characteristics are driving the drop in visits. To investigate this issue, we regressed the use of services on patient characteristics and the time trend variables, using only patients in the control agencies. We found that when we controlled for patient characteristics, the decline in the number of visits over time grew larger. The overall, unadjusted mean number of visits declined 4.3 visits from Year 1 to Year 3, but after controlling for patient characteristics, the mean number of visits declined 5.2 visits (see Table III.2). Thus, the majority of the decline in visits was due to an overall trend outside the demonstration, not to changes in patient characteristics.

In each year of the demonstration, the cumulative number of visits was significantly different between treatments and controls in each 30-day time period (Table III.3). Again, the difference in impacts between the demonstration years was not significant ($p = 0.78$ for Year 1 - Year 2 and p =

TABLE III.1

NUMBER OF VISITS PROVIDED IN THE AT-RISK PERIOD, TOTAL AND BY SERVICE TYPE[a]

| | Year 1 | | | Year 3 | | |
|---|---|---|---|---|---|---|
| Number of Visits | Treatment Group Mean[b] | Control Group Mean | Difference[b] (*p*-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (*p*-value)[c] |
| All | 37.6 | 45.3 | -7.7*** (0.00) | 32.0 | 41.0 | -9.0*** (0.00) |
| Skilled Nursing | 18.0 | 21.7 | -3.7*** (0.00) | 15.6 | 20.8 | -5.2*** (0.03) |
| Home Health Aide | 12.6 | 15.9 | -3.3*** (0.00) | 9.5 | 12.3 | -2.8*** (0.00) |
| Physical Therapy | 5.5 | 5.7 | -0.2 (0.72) | 5.4 | 5.9 | -0.5 (0.34) |
| Occupational Therapy | 0.8 | 1.0 | -0.2 (0.22) | 0.9 | 1.0 | -0.1 (0.13) |
| Speech Therapy | 0.3 | 0.3 | 0.0 (0.39) | 0.3 | 0.3 | 0.0 (0.95) |
| Medical Social Worker | 0.5 | 0.8 | -0.3*** (0.01) | 0.5 | 0.8 | -0.3*** (0.01) |
| Sample Size | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 12,451 | 11,641 | ----- |
| Agencies | 36 | 38 | | 36 | 38 | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Regression-adjusted (through ordinary least squares models) to control for preexisting differences between treatment and control agencies.

[c] The *p*-value corresponds to a test of whether the treatment-control difference (impact) is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE III.2

AVERAGE NUMBER OF VISITS BY YEAR PROVIDED BY CONTROL GROUP AGENCIES, CONTROLLING FOR PATIENT CHARACTERISTICS[a]

| Visit Type | Year I | Year 3 | Year I - Year 3 Difference (p-value) |
|---|---|---|---|
| All | 45.60 | 40.40 | -5.20*** (0.01) |
| Skilled Nursing | 21.90 | 20.30 | -1.60 (0.13) |
| Home Health Aide | 15.90 | 12.30 | -3.64*** (0.00) |
| Physical Therapy | 5.70 | 5.80 | 0.12 (0.64) |
| Occupational Therpy | 1.00 | 1.00 | 0.05 (0.67) |
| Speech Therapy | 0.30 | 0.20 | -0.08 (0.15) |
| Medical Social Worker | 0.80 | 0.80 | -0.05 (0.49) |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a]Reflects a patient's first admission to a home health agency participating in the demonstration.

* Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE III.3

CUMULATIVE NUMBER OF VISITS PROVIDED IN THE AT-RISK PERIOD[a]

| Total Visits from Admission Through: | Year 1 | | | Year 3 | | |
|---|---|---|---|---|---|---|
| | Treatment Group Mean[b] | Control Group Mean | Difference[b] (*p*-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (*p*-value)[c] |
| 30 days | 17.6 | 19.6 | -2.0*** (0.00) | 16.3 | 19.1 | -2.8*** (0.00) |
| 60 days | 27.2 | 31.3 | -4.1*** (0.00) | 24.3 | 30.0 | -5.7*** (0.00) |
| 90 days | 33.0 | 39.0 | -6.0*** (0.00) | 28.6 | 36.3 | -7.7*** (0.00) |
| 120 days | 37.6 | 45.3 | -7.7*** (0.00) | 32.0 | 41.0 | -9.0*** (0.00) |
| Sample Size | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 12,451 | 11,641 | ----- |
| Agencies | 36 | 38 | | 36 | 38 | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Regression-adjusted (through ordinary least squares models) to control for preexisting differences between treatment and control agencies.

[c] The *p*-value corresponds to a test of whether the treatment-control difference (impact) is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

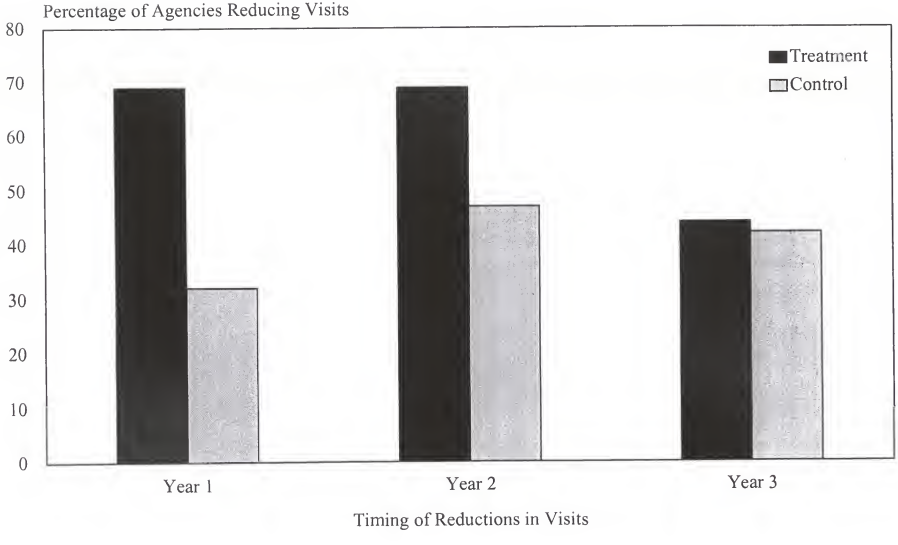
0.46 for Year 2- Year 3), meaning that both treatment and control agencies were cutting visits at an equal rate, which continued throughout the three demonstration years.

Although we have shown that prospective payment was successful in decreasing the total number of visits provided, it is important to understand whether all agencies were able to reduce their visits in response to the demonstration incentives, or whether the demonstration impacts were due simply to a few agencies making large cuts. We found that nearly all prospectively paid agencies reduced visits relative to their base year, with 94 percent reducing visits per episode by 3 percent or more over the course of the demonstration. As shown in Figure III.1, two-thirds of the prospectively paid agencies cut visits in Year 1 relative to their base year, and another two-thirds cut visits in Year 2 relative to their first-year practice pattern.

In contrast, 74 percent of the control agencies also reduced their visits per episode by 3 percent or more during the demonstration. But, as Figure III.1 shows, declines among the control agencies came much later in the demonstration and were not as pervasive as the declines among the prospectively paid agencies. Only one-third of the control agencies cut visits in Year 1, and just under half cut visits in Year 2. The smaller, but still significant, number of control agencies cutting visits later in the demonstration is consistent with the evidence that control agencies were responding to environmental pressures from outside the demonstration. Some of these pressures, such as the influence of managed care on the care of all patients, were consistently growing over the time period coinciding with the demonstration, while others, such as ORT, would have occurred during demonstration Years 2 and 3.

FIGURE III.1

REDUCTIONS IN VISITS PROVIDED BY DEMONSTRATION YEAR AND TREATMENT STATUS[a]




[a]Reduction in visits refers to a reduction of greater than three percent relative to previous year. Data for N = 36 treatment agencies and N = 38 control agencies.

## 2. Number of Visits by Type

Just as in the total number of visits provided, impacts on the number of visits by type remained remarkably stable. However, both treatment and control agencies continued to cut the number of visits they provided in each year of the demonstration (Table III.1).[2] The largest cuts were made in skilled nursing visits, with treatment group agencies cutting these visits by 13 percent, from 18.0 in Year 1 to 15.6 in Year 3. Control group agencies also reduced their skilled nursing visits, by 4 percent from 21.7 in Year 1 to 20.8 in Year 3.

Despite an even greater reduction in home health aide visits, the impact of prospective payment remained the same. Treatment group agencies were able to reduce the average number of home health aide visits they provided, from 12.6 in Year 1 to 9.5 in Year 3. Control group agencies also reduced the number of aide visits from 15.9 in Year 1 to 12.3 in Year 3. These reductions represent a 25 percent decrease in the number of aide visits provided over the demonstration. Again, however, treatment group agencies provided significantly fewer aide visits in each year.

In contrast to skilled nursing and aide visits, therapy visits of all types did not change significantly during the demonstration. Treatment and control agencies provided almost the same number of physical therapy visits in each year, and the number of these visits did not decrease from Year 1 to Year 3.[3] We found the same results for the demonstration's impact on occupational therapy and speech therapy visits.

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[2]We chose to simplify the presentation of impacts in this chapter by including the data for Years 1 and 3 only. The results for Year 2, which were similar to those of Years 1 and 3, are included in Appendix A.

[3]In our previous report, Trenholm found similar results for physical therapy visits in the at-risk period. However, when physical therapy visits were examined for months 5 to 8 and 9 to 12, treatment agencies cut a greater number of visits, resulting in a statistically significant difference.

The number of medical social worker visits provided was very small overall. Treatment group agencies provided an average of 0.5 medical social worker visits during the at-risk period in Year 1 while control group agencies provided 0.8 visits. This difference is statistically significant. Although an individual patient's probability of receiving a medical social worker visit did decline across the three years for treatment group agency patients (see Table III.4), the absolute number of these visits provided did not decrease significantly through the demonstration. Indeed, among those treatment group patients who did receive a medical social worker visit, the number of visits provided actually increased.

Finally, it is interesting to note that the overall composition of visits in the at-risk period was remarkably constant for both treatment and controls in each year of the demonstration. For example, among treatment agencies, skilled nursing visits accounted for 48 percent of total visits in Year 1, 50 percent in Year 2, and 49 percent in Year 3. The proportion of home health aide visits in treatment agencies was 33 percent of total visits in Year 1, 30 percent in Year 2, and 30 percent in Year 3. This result is particularly interesting because we had speculated that agencies may have dramatically cut visits by home health aides. We thought that agencies may have believed this semi-skilled care was overutilized and could easily be replaced by care from family members or other social supports. However, because therapy services are such a small part of the services rendered, even though utilization remained steady while other service fell, the overall proportion of visits did not change, even after three years' experience with prospective payment, aide visits were being provided in the same proportion as they were at the start of the demonstration.

### 3. Probability of Receiving Certain Types of Visits

Like the consistent decreases in the total number of visits provided and visits provided by type, generally the probability of receiving different types of visits decreased as well (Table III.4). The

TABLE III.4

PROBABILITY OF RECEIVING A GIVEN SERVICE DURING THE AT-RISK PERIOD, BY TREATMENT STATUS[a]

| Service | Year 1 | | | Year 3 | | |
|---|---|---|---|---|---|---|
| | Treatment Group Mean[b] | Control Group Mean | Difference[b] (*p*-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] |
| Skilled Nursing Visit(s) | 92.6 | 96.6 | -4.0*** (0.01) | 90.9 | 93.7 | -2.8** (0.05) |
| Home Health Aide Visit(s) | 44.4 | 46.7 | -2.3 (0.18) | 39.9 | 42.4 | -2.5 (0.24) |
| Any Therapy Visit(s) | 43.5 | 44.5 | -1.0 (0.70) | 48.0 | 49.8 | -1.8 (0.51) |
| Physical Therapy Visit(s) | 42.6 | 42.6 | 0.0 (0.99) | 46.9 | 47.8 | -0.9 (0.73) |
| Occupational Therapy Visit(s) | 7.7 | 12.5 | -4.8*** (0.01) | 9.2 | 13.9 | -4.7* (0.07) |
| Speech Therapy Visit(s) | 2.7 | 3.3 | -0.6 (0.08) | 2.4 | 3.4 | -1.0 (0.20) |
| Medical Social Worker Visit(s) | 25.8 | 29.2 | -3.4 (0.21) | 19.0 | 29.3 | -10.3*** (0.01) |
| Sample Size | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 12,451 | 11,641 | ----- |
| Agencies | 36 | 38 | | 36 | 38 | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Regression-adjusted (through logit models) to control for preexisting differences between treatment and control agencies.

[c] The *p*-value corresponds to a test of whether the treatment-control difference (impact) is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

probability of receiving a skilled nursing visit decreased slightly, but the impacts did not change significantly across the three years of the demonstration. Patients in treatment group agencies had a 92.6 percent likelihood of receiving a skilled nursing visit in Year 1, while patients in control group agencies had a 96.6 percent chance. By Year 3, the probability of receiving a skilled nursing visit had declined to 90.9 percent in treatment agencies and 93.7 percent in control agencies. This decrease is likely to be the result of agencies' using physical therapists to open the cases of patients who needed therapy only.

For medical social worker visits, there was a large decrease in the probability of a visit in Year 3 for treatment group agencies. Although the probability of a medical social worker visit was lower among treatment group agency patients for both Year 1 and Year 2, the differences of 4 to 5 percent were not statistically significant. However, in Year 3, the probability of a medical social worker visit in a treatment group agency was 19.0 percent versus 29.3 percent in a control group agency. This 10-percentage-point difference was highly significant; it suggests that treatment group agencies, seeking to cut any excess visits, had, by Year 3, grown much more selective in deciding which patients received medical social work visits. As discussed previously, however, those patients who did receive a medical social worker visit were actually provided more of these visits in Year 3 than in Year 1.

Unlike skilled nursing or medical social work visits, the likelihood of receiving either physical or occupational therapy visits increased for both treatment and control group agency patients, but the change in demonstration impacts was not statistically significant. However, the change in probability from Year 1 to Year 3 of receiving any type of therapy visit was significant for treatment and control group agencies ($p = 0.04$ and $p = 0.03$, respectively).

## 4. Episode Length

Consistent with the findings on service use, episode lengths fell sharply for both treatment and control agencies over the course of the demonstration. Episode lengths for treatment group agencies fell more sharply, however, leading to only modest changes in impact over time (Table III.5). For example, by 30 days after admission, the proportion of episodes completed by treatment agencies rose sharply from 33 percent in Year 1 to 40.4 percent in Year 3.[4] However, this rise was largely offset by an increase of 5.1 percentage points by control agencies (from 26.6 percent to 31.7 percent). The estimated impact of prospective payment on episode completion by 30 days thus rose only marginally ($p = 0.08$), from a 6.4 percentage point difference in Year 1 to 8.7 in Year 3. Similar behavior was observed for later points from admission (between 31 and 60 days), although the impacts on episode completion actually weakened slightly and became statistically insignificant when measured at 90 and 120 days. Thus, although control group agencies began to catch up to the treatment group agencies, in terms of how quickly they were discharging their patients, they were still significantly behind the treatment agencies at the end of Year 3.

Perhaps more interesting than the impact findings on episode lengths is the striking proportion of episodes being completed within the at-risk period by Year 3. In Year 1, 74 percent of treatment group agency patient episodes were completed by the time the at-risk period ended. By Year 3, this percentage had risen to 86 percent. This decline in episode lengths reflects significant changes in the home health care environment. First, the restrictions imposed by the BBA legislation on services for patients who required only venipuncture certainly added to the decline in the use of services. Furthermore, the proportion of patients under managed care contracts grew in these agencies and

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[4]In an additional analysis (data not shown), we examined if this acceleration in the rate of discharge could have been due to treatment agencies having a higher percentage of episodes consisting of two visits or less. Treatment-control differences were insignificant, leading us to believe that treatment agencies truly were discharging patients more quickly overall.

TABLE III.5

CUMULATIVE PERCENTAGE OF EPISODES ENDED IN EACH MONTH OF THE AT-RISK PERIOD, BY TREATMENT STATUS[a]

| Time Since Admission: | Year 1 | | | Year 3 | | |
|---|---|---|---|---|---|---|
| | Treatment Group Mean[b] | Control Group Mean | Difference[b] (*p*-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (*p*-value)[c] |
| 0-30 days | 33.0 | 26.6 | 6.4*** (0.00) | 40.4 | 31.7 | 8.7*** (0.00) |
| 31-60 days | 56.8 | 49.2 | 7.6*** (0.00) | 68.9 | 58.8 | 10.1*** (0.00) |
| 61-90 days | 66.3 | 58.8 | 7.5*** (0.00) | 77.3 | 70.5 | 6.8*** (0.00) |
| 91-120 days | 74.0 | 66.3 | 7.7*** (0.00) | 86.0 | 79.5 | 6.5*** (0.00) |
| Sample Size | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 12,451 | 11,641 | ----- |
| Agencies | 36 | 38 | | 36 | 38 | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patients) weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Means for treatment agencies have been regression adjusted (through logit models) to control for preexisting differences between patients or agencies.

[c] The *p*-value corresponds to a test of whether the treatment-control difference (impact) is statistically significant from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

managed care organizations tend to have very tight restrictions on their home health services. Lessons learned from the managed care environment inevitably influence the care of patients under Medicare fee-for-service as well (Cheh et al., forthcoming).

To summarize, we found continued decreases in the number of visits provided by treatment group agencies. These prospectively paid agencies also continued to shorten the length of patient episodes. Although control agencies made similar reductions in visits and length of service, impacts were maintained over the three years of the demonstration.

## D. SUBGROUP IMPACTS

Agencies may respond differently to the demonstration incentives. To the extent that they do, these responses have different implications for how a nationwide prospective payment system will work. Our previous report found that agencies with relatively high-use practice patterns prior to the demonstration reduced visits significantly more than agencies with low-use practice patterns. Although both types of agencies significantly reduced total visits in the patient-year, high-use agencies cut about twice the number of visits as did low-use agencies; in particular, they were able to reduce the number of home health aide visits. In general, the high-use agencies seem to have made larger cuts in services simply because they had more "room" to do so. Our previous report (Trenholm 2000) found other significant differences in impacts between the agency-level subgroups defined by size, for-profit status, and other characteristics, but all these differences were largely or entirely attributable to the differences in practice patterns.

In the present analysis, we found that the differences in impacts between high-use and low-use agencies shrank dramatically during the demonstration period (see Table III.6). In Year 1, both agency types provided significantly fewer visits in the at-risk period, but high-use agencies provided

TABLE III.6

IMPACT OF PROSPECTIVE PAYMENT ON SERVICE USE. BY WHETHER THE AGENCY HAS A HIGH-USE OR LOW-USE PRACTICE PATTERN[a]

| | Year 1 | | | Year 3 | | |
|---|---|---|---|---|---|---|
| | Treatment Group Mean[c] | Control Group Mean | Difference[c] (*p*-value)[d] | Treatment Group Mean[c] | Control Group Mean | Difference[c] (*p*-value)[d] |
| **Service Use During the At-Risk Period[b]** | | | | | | |
| Total Visits | | | | | | |
| High-volume practice pattern | 41.6 | 52.5 | -10.9*** | 36.4 | 47.1 | -10.7*** |
| Low-volume practice pattern | 28.9 | 31.6 | -2.7*** | 22.0 | 29.4 | -7.4*** |
| Subgroup Difference | | | -8.2 (0.01) | | | -3.3 (0.37) |
| Skilled Nursing Visits | | | | | | |
| High-volume practice pattern | 19.4 | 25.1 | -5.7*** | 17.1 | 24.3 | -7.2*** |
| Low-volume practice pattern | 14.3 | 15.0 | -0.7 | 11.5 | 14.1 | -2.6** |
| Subgroup Difference | | | -5.0 (0.01) | | | -4.6 (0.05) |
| Home Health Aide Visits | | | | | | |
| High-volume practice pattern | 14.7 | 19.7 | -5.0*** | 11.6 | 15.2 | -3.6*** |
| Low-volume practice pattern | 8.2 | 8.5 | -0.3 | 4.5 | 6.7 | -2.2 |
| Subgroup Difference | | | -4.7 (0.00) | | | -1.4 (0.51) |
| Physical Therapy Visits | | | | | | |
| High-volume practice pattern | 5.6 | 5.5 | 0.1 | 5.8 | 5.7 | 0.1 |
| Low-volume practice pattern | 5.3 | 6.0 | -0.7 | 5.0 | 6.4 | -1.4* |
| Subgroup Difference | | | 0.8 (0.44) | | | 1.5 (0.23) |
| **Probability of Last Visit by Day 120[b]** | | | | | | |
| High-volume practice pattern | 72.4 | 62.3 | 10.1*** | 85.4 | 77.1 | 8.3*** |
| Low-volume practice pattern | 78.1 | 74.2 | 3.9** | 88.5 | 84.1 | 4.4 |
| Subgroup Difference | | | 6.2 (0.06) | | | 3.9 (0.33) |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a]An agency is defined as having a high-use practice pattern if its (case-mix adjusted) number of visits per episode in the base (predemonstration) period was above the median for all agencies in the sample. Otherwise, it is defined as having a low-use practice pattern.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c]Regression-adjusted (through ordinary least squares and logit models) to control for pre-existing differences between treatment and control agencies.

[d]The *p*-value corresponds to a test of whether the subgroup difference is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

11 fewer visits compared to the high-use control group agencies, while low-use agencies provided 3 fewer visits relative to the control low-use agencies--a difference of 8 visits. By Year 3, high-use treatment agencies still provided about 11 fewer visits, but the impact on low-use agencies more than doubled to over 7 visits per during the at-risk period. The difference in impact from Year 1 to Year 3 fell from 8 visits to an insignificant 3 visits, meaning that, in percentage terms, low-use agencies cut visits more sharply over the demonstration.

The driving factor behind this change was the aggressive cutting of visits by low-use, prospectively paid agencies. While all types of agencies reduced their visits per episode during this period, low-use prospectively paid agencies reduced their average visits per episode from 29 to 22 (23 percent), whereas low-use control agencies reduced average visits per episode from 32 to 29 (7 percent). In contrast, both treatment and control high-use agencies cut their average visits per episode by five visits during the same period.

Examining other measures of service use, we find a similar pattern--the differences in impacts that we observed in Year 1 for high-use and low-use agencies are not as apparent in Year 3 with the exception of skilled nursing visits. All the agencies reduced skilled nursing visits and home health aide visits, as well as the length of the episode. But the low-use treatment agencies reduced services slightly more, thereby "catching up" to the high-use treatment agencies. An exception to this pattern is physical therapy services, where we found no differences either in Year 1 or in Year 3, nor did we find any impacts of prospective payment until Year 3 and even then, it was only among the low-use agencies. This pattern reflects the tight labor market that existed for physical therapists at the start of the demonstration (Cheh et al., forthcoming). Since many of the agencies—both treatment and control—did not have enough physical therapists, the number of visits was constrained. Thus, agencies may not have had the opportunity to reduce services, as they did in other disciplines.

The change in the practice pattern subgroup estimates led us to investigate whether the subgroups (defined by size, for-profit status, and other characteristics), also changed in the last year of the demonstration. To explore this issue, we estimated selected impacts over the course of the demonstration for subgroups defined by size and profit-status, controlling for differences in impacts between agencies with high- and low-use practice patterns.[5] Consistent with prior findings, we find no significant differences in impacts between these other subgroups, once differences in practice patterns have been taken into account. This suggests that all types of home health agencies will have the ability to reduce visits under a national system of prospective payment.

## E. ROBUSTNESS TESTS

We constructed several alternate models to examine whether our estimates of demonstration impacts were dependent on particular observations or statistical procedures (results from these tests are presented in this section). First, we investigated differences in impacts for selected outcomes (the total number of visits; the number of visits by skilled nurses, home health aides, and physical therapists; and the length of service receipt), using regression models without sample weights. These unweighted estimates place more emphasis on the behavior of larger agencies, rather than place equal emphasis on that of all agencies. Second, we examined the sensitivity of our results to outliers (that is, agencies with extreme values for particular outcomes). Finally, we examined how impacts differed when service use data on all of a patient's admissions were included.[6]

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[5]See Appendix A for a summary of these results. We focused on subgroups defined by size and for-profit status because the corresponding subsamples are large enough to be explored in models that include multiple interactions between treatment status, time, and subgroup status. This is not true of subgroups defined by auspice, for example, which includes a subsample of only nine hospital-based agencies (three treatment and six control).

[6]Recall that, in constructing the main data file for the impact analysis, we chose to include only the first 120-day at-risk period the patient had during the demonstration. We did this so that our (continued...)

The impacts from unweighted samples are somewhat smaller than those from the weighted samples, but they remain large (Table III.7). Moreover, the impact (difference), as a percentage of the control group mean, is nearly identical for the weighted and unweighted samples in both Year 1 and Year 3. For example, in Year 1, the impact on total visits is approximately six visits in the unweighted sample and eight visits in the weighted sample, which correspond to 16 percent and 17 percent of their respective control group means. In Year 3, the impact on total visits is 7.4 visits in the unweighted sample and 9 visits in the weighted sample, which correspond to 23 percent and 22 percent of their respective control group means. The impacts from the unweighted sample remain significant and substantial over the course of the demonstration; they do not suggest that our findings have been distorted by placing disproportionately high weight on admissions to smaller agencies.

To investigate whether our estimates have been unduly influenced by agency-level outliers, we dropped the four treatment and four control agencies with the two highest and two lowest values in each group, then reestimated the selected regressions on visits. The estimated impacts of prospective payment under this alternative model are similar to those found in the main results (see Table III.7). For each outcome measure, the estimated impact changed little in size or statistical significance when outlier agencies were dropped. In both Year 1 and Year 3, the effect on total visits is slightly larger than in the main impact results, whereas the impact on the probability of completing services within the first 120 days is slightly smaller.

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[6](...continued)
impact estimates would be unaffected by the possible correlation between treatment status and subsequent episode starts. The choice to exclude subsequent episodes may have resulted in a patient population in Year 3 that is quite different in terms of patient characteristics such as comorbid conditions and functional status from that in Year 1. By adding subsequent episodes back into the analysis, we will examine whether these differences had an effect on demonstration impacts.

TABLE III.7

IMPACTS OF PROSPECTIVE PAYMENT UNDER ALTERNATE MODELS[a]

| | Year 1 | | | | Year 3 | | | |
|---|---|---|---|---|---|---|---|---|
| | Main Analysis | Impacts Under Alternative Models | | | Main Analysis | Impacts Under Alternative Models | | |
| Outcome | Difference[b] (*p*-value)[c] | Unweighted (*p*-value)[c] | Outliers: Impact with 80 Percent Agency Subsample (*p*-value)[c] | Subsequent Episodes Included (*p*-value)[c] | Difference[b] (*p*-value)[c] | Unweighted (*p*-value)[c] | Outliers: Impact with 80 Percent Agency Subsample (*p*-value)[c] | Subsequent Episodes Included (*p*-value)[c] |
| **Service Use During the At-Risk Period** | | | | | | | | |
| Total Visits | -7.7 (0.00) | -5.9 (0.00) | -8.4 (0.00) | -7.6 (0.00) | -9.0 (0.00) | -7.4 (0.00) | -9.7 (0.00) | -10.1 (0.00) |
| Skilled Nursing Visits | -3.7 (0.00) | -3.8 (0.00) | -4.0 (0.00) | -3.6 (0.00) | -5.2 (0.00) | -3.9 (0.00) | -4.3 (0.00) | -5.6 (0.00) |
| Home Health Aide Visits | -3.3 (0.00) | -1.2 (0.13) | -2.2 (0.03) | -3.2 (0.01) | -2.8 (0.02) | -2.0 (0.02) | -1.7 (0.15) | -3.3 (0.01) |
| Physical Therapy Visits | -0.2 (0.72) | -0.3 (0.66) | -0.1 (0.76) | -0.2 (0.67) | -0.5 (0.46) | -0.7 (0.25) | -0.8 (0.11) | -0.6 (0.36) |
| **Probability of Last Visit by Day 120** | 7.7% (0.00) | 7.1% (0.00) | 7.5% (0.00) | 7.5% (0.00) | 6.5% (0.00) | 3.2% (0.03) | 6.0% (0.01) | 7.0% (0.00) |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a]Reflects a patient's first admission to a home health agency participating in the demonstration.

[b]Regression-adjusted (through ordinary least squares and logit models) to control for preexisting differences between treatment and control agencies.

[c]The *p*-value corresponds to a test of whether the treatment-control difference is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

To examine the influence of excluding subsequent patient episodes on service use impacts, we added these episodes back into the analysis file and reestimated the regression model.[7] The estimated impacts under this alternative model are very similar to those in the main analysis (see Table III.6). This model produced nearly identical impacts to those in the main model for both Year 1 and Year 3 data.

A final concern is that exclusion of certain agencies from our analysis (see Chapter II for a discussion of excluded agencies) may have biased our results. Our previous report (Trenholm 2000) found no evidence that the exclusion of these agencies explains the large and consistent impacts on service use seen in Year 1. In addition, only a small number of other agencies were dropped from the present report, and they were equally distributed between the treatment and control groups. Thus, there is little reason to believe that their exclusion from the data should have a strong impact on our findings.

In summary, the various robustness checks confirm the stability of our impact estimates. The estimated effects of prospective payment are large and significant regardless of how we weighted the observations or which agencies were included.

## F. SUMMARY

Our analysis of the demonstration impacts found that prospective payment resulted in broad-based and substantial reductions in the number of home health visits provided and the length of the patient episode. These impacts were sustained across the three years of the demonstration, and possibly grew slightly over time.

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[7]In reestimating this model, we also had to delete the control variable that accounted for home health use in the previous six months, since this variable would have been highly correlated with treatment/control status.

Key results include:

- The total number of visits provided by treatment group agencies in the at-risk period decreased nearly 15 percent by Year 3, from 37.6 visits in Year 1 to 32.0 visits in Year 3. However, the impacts from prospective payment remained relatively stable as control agencies also substantially reduced service use.

- The largest reductions in visits were made in skilled nursing and home health aide visits.

- The overall proportions of the different types of visits provided remained constant, because reductions in the number of visits provided were made proportionately across all visit types.

- The length of a patient episode continued to decrease into Year 3, with 40.4 percent of treatment episodes ending by the 30th day of admission in Year 3 and 85.5 percent ending by the completion of the at-risk period. But, again, the difference between treatment and control agencies remained stable, since control agencies also reduced their episode lengths.

- The differential impacts found between high-use and low-use agencies earlier in the demonstration diminished, as prospectively paid agencies with low-use service patterns aggressively reduced service use.

Our results show that control group agencies significantly reduced the number of visits per episode in response to factors outside the demonstration. Despite this significant decline, the prospectively paid agencies still provided significantly fewer visits--22 percent fewer visits in the last year of the demonstration. This demonstrates that, in comparison to a cost-based reimbursement system, prospective payment offers a substantial opportunity to reduce service use.

# IV. ANALYSIS OF LONG-STAY PATIENTS

Long-stay home health patients, those who have been in care longer than one year, have become an important subset of the Medicare home health patient population; they are a significant percentage of the population and consume a disproportionate number of the total home care visits provided (Freedman 1999). The implementation of a national prospective payment system may have a major effect on the care of these patients. The demonstration design (with per-visit outlier payments after 120 days) did not provide clear incentives to limit care for this population. However, it does provide us with an opportunity to examine the characteristics of individuals who became long-stay patients in agencies operating under prospective payment or cost-reimbursement.

Our analysis addresses two questions related to long-stay patients and their care. First, will long-stay patients continue to make up a significant percentage of the total home health population? In the late 1980s, changes in the interpretation of the Medicare home health benefit guidelines made home health services available to more beneficiaries for a longer period of time. This caused a shift in home health clientele to longer-term users. Freedman (1999) examined data from the 1992 National Home and Hospice Care Survey and found that at 12 months after admission, 15 percent of all the patients in this study were still receiving care and that 8 percent stayed in care for two years or longer. However, since prospective payment does provide an incentive to limit care, it will be important to understand how this incentive may affect the number of long-stay patients and the characteristics of those who remain in care. Second, will long-stay patients continue to consume a disproportionate number of the home health visits provided? Freedman (1999) found that, while long-stay patients were only 15 percent of the home care population, they represented 50 percent of home care patient months. In addition, Welch, Wennberg, and Welch (1996) reported that 61

percent of all home health visits were to enrollees who received home health for 6 months or more and that 24 percent of all visits were to patients who received care for 12 months or more. Even if the same proportion of patients remain in care as long-stayers under prospective payment, and if the number of visits decreases, these patients may be a much less significant component of Medicare home health.

It would have been interesting if we were able to estimate the impact of prospective payment on service use by long-stay patients. However, the demonstration was not designed with this purpose in mind, nor are we able to use the available data to estimate these impacts since the long-stayer sample-selection criterion is endogenous to the treatment group. The main analysis file for this report contains data for all treatment and control group patient episodes that, because of random assignment, can be used to estimate demonstration impacts on service use. Trenholm (2000) found that the payment method had an impact on the length of a patient's home health episode; that is, patients in prospectively paid agencies were discharged more quickly than patients in cost-reimbursed agencies. When we created the long-stay analysis file, we selected only those patients who remained in home health care for more than one year. Thus, we have a differentially selected group of patients that is no longer random across the treatment and control groups. These differences are the focus of our analysis because they may influence the design of a payment policy for patient outliers.

## A. THE LONG-STAY SAMPLE

For this analysis, we defined long-stay patients as those whose length of stay in home care was greater than 365 days in a single episode. As discussed in Chapter II, we included in our analysis sample patients who were admitted between January 1, 1996 and December 31, 1996. This

permitted us to follow these patients for two years or until their discharge from home care, whichever came first.

Unlike the impact analysis in Chapter III, we chose to conduct the analysis of long-stay patients on an unweighted sample. The advantage of this approach is that it may provide a more representative view of the long-stay patient population. Because we are not measuring impacts, it becomes less important to weigh agencies equally, and more relevant to present the differences at the patient level. The disadvantage of this approach is that, if a small number of agencies have a large number of long-stay patients relative to other agencies, these agencies will contribute disproportionately to the results.

Using the inclusion criteria defined previously, we identified 3,311 long-stay patients for analysis. All 74 home health agencies in the analysis sample had at least one long-stay admission. The number of long-stay patients per agency ranged from 557 to 1; and the percentage of long-stay patients, in terms of the agency population as a whole, ranged from 34 percent to 1 percent. Long-stay patients comprised, on average, approximately 7 percent of total home care admissions for calendar year 1996.[1]

Patients were equally distributed between treatment (N = 1,685) and control (N = 1,626) agencies. In a few agencies, however, there was considerable clustering of long-stay patients. The

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[1]In our previous report (Trenholm 2000), we reported that 10 percent of patients in treatment group agencies and 15 percent of patients in control group agencies continued to receive care more than one year after admission. The lower percentages given in the current report can be attributed to the difference in the unit of analysis used. In Trenholm, the unit of analysis was the patient year in which patients were followed for one year after their first admission to home care. These patients may have had multiple episodes (readmissions) within this year. In this report, we use the patient's first natural episode as the unit of analysis, meaning that only the patient's first episode of care (the time from admission to discharge, or a 45-day gap in care) is considered.

long-stay patients of one large treatment agency comprised 17 percent of the total long-stay sample and 33 percent of the prospectively paid long-stay sample.

## B. RESULTS

Our analysis examines: (1) the number of long-stay patients and the proportion of services used for their care, and (2) the characteristics of long-stay patients.

### 1. Did Long-Stay Patients Remain a Large Part of Home Care?

Because the demonstration led to significant reductions in the length of home health episodes, one would expect long-stay patients to become a very small part of the caseload. However, we found that long-stay patients were still a substantial part of the agencies' admissions, and that they continued to utilize a disproportionate number of home health services. Long-stay patients made up 6.5 percent of all home health patients admitted to prospectively paid agencies. While these numbers are not as high as those reported by Freedman (who reported that 15 percent remained in care for more than a year), they are still substantial and are higher than those reported before the Duggan decision (Duggan v. Bowen 1988), which led to escalation of the use of long-term home health care (Branch et al. 1993). Visits to long-stay patients constituted 35 percent of all visits provided by prospectively paid agencies to all patients admitted in this period.[2] This suggests that, despite reductions in the length of episodes, long-term stayers will remain an important component of Medicare home health even after the new payment system is implemented.

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[2]In the calculation of the proportion of services used, we measured all service use (the denominator) as the total number of visits to all patients admitted between January 1, 1996 and December 31, 1997. This includes both "short-stay" patients who were discharged within one year of admission and "long-stay" patients who had a length of stay of more than one year. We counted all visits to these patients, including visits to patients who were admitted, discharged, and readmitted within the 24-month follow-up period (until December 31, 1997).

Although our estimate of the percent of long-stay patients in treatment group agencies is higher than that reported before the Duggan decision, it is surprisingly similar to the pattern found in the control group, where 7.3 percent of the patients accounted for 35 percent of the total visits. Thus, the demonstration prospective payment system had little effect on the length of care for those continuously enrolled in care.

How, then, could we have reported previously that the demonstration's prospective payment system had an impact on length of stay up to one year after admission (Trenholm 2000)? The reason is, the impact analysis in Trenholm (2000) used a fixed length of time to analyze impacts, during which a patient could be discharged and readmitted; whereas the present analysis examines patients who are continuously in care. Although treatment and control group agencies had approximately equal rates of readmission--10.7 and 10.2 percent, respectively--treatment group agencies reduced the length of the patient episode by approximately 25 days in the year after admission. Thus, the combined effect of these two findings is that patients in cost-reimbursed agencies were more likely to be in care (but not continuously) at one year after admission. From this, we conclude that prospective payment has differential effects on length of stay; significantly decreasing the length of stay for patients who require shorter-term care and having little effect on the length of stay for that subset of patients who require continuous care.

Furthermore, we found that, at the individual agency level, the importance of long-stay patients in the caseload varied greatly. For the majority (54 percent) of the prospectively paid agencies, long-stay patients represent less than 6 percent of their caseload, with a few agencies (8 percent) having less than 2 percent of their patients as long-stay patients. For some agencies, however, long-term stayers represented more than 15 percent of their caseloads, with one agency having 26 percent

of its patients remain in care for one year or more. Thus, for some agencies, the future of the long-term care patient under Medicare home health will have a major effect on their operations.

## 2. What Were the Characteristics of Long-Stay Patients?

Given the small difference in the proportion of patients that became long-stay patients, one would expect their characteristics to be the same. Indeed, the demographics, health resource use, and diagnoses were similar across prospectively paid and cost-reimbursed agencies, as well as those reported previously.

The average age of patients was essentially the same--78--as the distribution across age groups, with approximately 8.5 percent under age 65, and 27 percent age 85 and older (Table IV.1). The majority (78 percent) of both types of patients were white; there were slightly more Hispanics and fewer blacks among the prospectively paid agencies, but these differences were insignificant, as were the differences in Medicaid status. The only difference that even approached statistical significance was a three-percentage-point difference in the proportion of females--prospectively paid agencies had slightly more females in their long-stay populations.

These findings are similar to those of Freedman (1999), who described long-stay patients from the National Home and Hospice Care Survey. In this survey, 68 percent of long-stay patients were female, and 24 percent were over the age of 85. Thus, the demographic characteristics of long-stay home health patients seems to be similar regardless of the time period or payment method under which they are identified.

Long-stay patients in prospectively paid, cost-reimbursed agencies were also similar in terms of their health care resource utilization measures (Table IV.2). Approximately 31 percent of all long-stay patients were admitted to home health from the hospital. Total Medicare Part A

TABLE IV.1

DEMOGRAPHIC CHARACTERISTICS OF LONG-STAY PATIENTS

| | Prospectively Paid (Percent) | Cost-Reimbursed (Percent) | p-value |
|---|---|---|---|
| Age | | | |
| Less than 65 | 8.8 | 8.4 | 0.70 |
| 65 to 74 | 23.3 | 24.8 | 0.56 |
| 75 to 84 | 40.4 | 39.2 | 0.48 |
| 85 and over | 27.5 | 27.6 | 0.96 |
| Mean Age | 78.1 | 77.8 | 0.69 |
| Female | 68.6 | 65.5 | 0.11 |
| Race | | | |
| White | 78.2 | 78.9 | 0.93 |
| Black | 15.4 | 17.8 | 0.75 |
| Hispanic | 2.9 | 1.7 | 0.30 |
| Asian | 1.1 | 0.4 | 0.14 |
| Other | 1.6 | 0.6 | 0.03 |
| Unknown | 0.8 | 0.6 | 0.31 |
| Medicaid-Eligible at Admission | 28.0 | 23.9 | 0.44 |
| **Sample Size** | **1,685** | **1,626** | |

SOURCE: Medicare claims data.

TABLE IV.2

RESOURCE USE CHARACTERISTICS OF LONG-STAY PATIENTS

| Characteristic | Prospectively Paid | Cost-Reimbursed | p-value |
|---|---|---|---|
| Patient Admitted to Home Health After a Hospital Stay | 31% | 31% | 0.99 |
| Amount of Part A Reimbursement in Previous 6 Months (Dollars) | $11,790 | $11,320 | 0.60 |
| Number of Days Hospitalized During Home Care Episode | 18.8 | 18.2 | 0.73 |
| **Sample Size** | **1,685** | **1,636** | |

SOURCE: Medicare claims data.

reimbursement in the six months prior to the home health admission was similar for patients in prospectively paid and cost-reimbursed agencies, averaging $11,790 and $11,320, respectively--an insignificant difference of 4 percent. During their home health episode, patients in both prospectively paid and cost-reimbursed agencies were hospitalized for slightly more than 18 days.

The long-stayers also had similar, principal diagnoses on admission to home health (Table IV.3). Common principal diagnoses included diabetes, congestive heart failure, cerebrovascular disease, chronic obstructive pulmonary disease, and diseases of the musculoskeletal system and connective tissues. These chronic medical conditions may indicate the need for continuing home health care for many patients, and they were just as frequent among both groups of patients. Our data are similar to those of Freedman (1999), who reported that among home health patients in care for more than 12 months, 21.7 percent had a diagnosis of arthritis on admission, 17.9 percent had diabetes, and 17.9 percent had hypertension.

## C. SUMMARY

The findings from our analysis of long-stay patients are important in the context of the implementation of a national prospective payment system for home health care. First, even for agencies that had begun to operate in a prospective payment mind-set, long-stay patients continued to represent a significant proportion (6.5 percent) of their home health patient population. This is not all that surprising, given that long-stay patients were present in the home health patient population even prior to the late 1980s and the Duggan decision, which spurred their growth (Branch et al. 1993). Second, visits to long-stay patients constituted 35 percent of all visits to all patients admitted in this period in both prospectively paid and cost-reimbursed agencies. Third, the demographic, health resource use, and diagnostic group characteristics of long-stay patients were similar across prospectively paid and cost-reimbursed agencies.

TABLE IV.3

PERCENT OF LONG-STAY PATIENTS WITH SELECTED COMMON CONDITIONS AS PRIMARY DIAGNOSIS AT ADMISSION

| Diagnostic Category | Prospectively Paid (Percent) | Cost-Reimbursed (Percent) | p-value |
|---|---|---|---|
| Infectious and Parasitic Diseases | 1.0 | 0.9 | 0.82 |
| Neoplasms | 3.7 | 3.9 | 0.81 |
| Endocrine, Nutritional. and Metabolic Diseases and Immunity Disorders | | | |
| Diabetes mellitus | 8.5 | 8.9 | 0.76 |
| Other | 1.6 | 1.5 | 0.93 |
| Diseases of the Blood and Blood-Forming Organs | | | |
| Pernicious anemia | 2.3 | 1.3 | 0.12 |
| Other | 1.9 | 2.0 | 0.84 |
| Mental Disorders | 2.7 | 2.6 | 0.93 |
| Diseases of the Nervous System and Sense Organs | 5.2 | 4.3 | 0.20 |
| Diseases of the Circulatory System | | | |
| Essential hypertension | 5.8 | 8.3 | 0.27 |
| Congestive heart failure | 6.8 | 7.0 | 0.77 |
| Acute, ill-defined cerebrovascular disease | 5.2 | 5.9 | 0.64 |
| Other | 16.0 | 14.8 | 0.47 |
| Diseases of the Respiratory System | | | |
| Chronic obstructive pulmonary disease | 4.2 | 5.2 | 0.27 |
| Other | 3.9 | 2.4 | 0.01 |
| Disease of the Digestive System | 2.3 | 3.0 | 0.15 |
| Disease of the Genitourinary System | 3.1 | 3.5 | 0.54 |
| Diseases of the Skin and Subcutaneous Tissues | | | |
| Chronic ulcer of the skin | 3.8 | 3.4 | 0.66 |
| Other | 2.0 | 1.6 | 0.35 |
| Diseases of the Musculoskeletal System and Connective Tissue | 5.4 | 6.6 | 0.30 |
| Symptoms. Signs. and Ill-Defined Conditions | | | |
| Incontinence of urine | 0.8 | 0.6 | 0.82 |
| Retention of urine | 0.6 | 0.6 | 0.92 |
| Other | 4.5 | 4.8 | 0.74 |
| Injury and poisoning | 8.5 | 6.7 | 0.15 |

SOURCE: Medicare claims data.

These results suggest that this is a core group of Medicare home health users, whose characteristics remain stable over time and who continue to use long-term home health care even if there are financial incentives for earlier discharge. Although long-stay patients are less than 10 percent of the home health population, our data suggest that they use one-third of all home health visits. Furthermore, these patients are not equally distributed across agencies. This suggests that it will be prudent to monitor how the new national payment system works for these patients, since significant deviations of payments from actual service use could result in significant profit or loss for the small group of agencies caring for this vulnerable group of patients.

Our analysis of long-stay patients has two limitations. First, the incentives of the demonstration are not the same as those under the actual national prospective payment system, in which agencies receive a case mix-adjusted payment for every 60-day episode of care, no matter how long the patient continues to receive services. Second, we were able to examine the characteristics of long-stay patients only at the time of their admission. We cannot assess changes in the makeup of the long-stay patient population over the course of the follow-up period, a factor that may play an important role in service use.

# V. DISCUSSION OF FINDINGS

A thorough analysis of the data from the first two years of the Per-Episode Home Health Prospective Payment Demonstration found strong, consistent evidence that prospective payment generated substantial, broad-based reductions in the number and duration of services (Trenholm 2000). This follow-up report, which analyzed the third year of the demonstration data, found that these reductions continued into the third year of the program. Even as cost-reimbursed agencies reduced their visits in response to forces outside the demonstration, the prospectively paid agencies reduced their service use by the same amount. Thus, the estimates from the earlier data remain the same. Hence, the results should be helpful to policymakers as they implement the national prospective payment system mandated by the Balanced Budget Act of 1997.

## A. KEY FINDINGS

The impacts of the demonstration remained constant during the three-year demonstration. In the first year of the demonstration, prospectively paid agencies provided an average of 37.6 visits during the at-risk period, while the cost reimbursed agencies provided 45.3--for an impact of 7.7 visits. By Year 3 of the demonstration, the prospectively paid agencies reduced their service use to 32 visits per episode. Cost-reimbursed agencies also reduced their service use to 41 visits in response to factors outside the demonstration, including the growth of managed care, implementation of Operation Restore Trust, and anticipation of future prospective payment.

The impact on total visits is primarily driven by reductions in skilled nursing and home health care visits, regardless of the time period. During the first year of the demonstration, prospectively paid agencies provided 18 skilled nursing visits per at-risk period--almost 4 visits fewer than the 21.7 visits rendered by the cost-reimbursed agencies. By Year 3, prospectively paid agencies reduced the

number of skilled nursing visits to 15.6, while the cost-reimbursed agencies cut their visits to 20.8--resulting in a difference of just over 5 skilled-nursing visits. A similar pattern can be found in home health aide care--where, throughout the demonstration, prospectively paid agencies consistently provided about 3 visits less per at-risk period. In contrast, the impacts on the number of therapy visits were small and insignificant and changed little over the course of the demonstration. The impacts on medical social worker visits were also unchanged.

The continued reduction in the number of visits was in part due to the continued decrease in the length of the episode. By the end of the demonstration, 40 percent of the prospectively paid episodes ended within 30 days of admission--a 7 percentage point increase from the first year of the demonstration. In contrast, by the end of the demonstration, 31.7 percent of cost-reimbursed episodes ended within 30 days of admission; this, however, is an 8.7 percentage point difference from the first year of the demonstration.

We also examined the characteristics of patients who remain in long-term care under the prospective payment system. Although the incentive to discharge patients and reduce service use after the episode payments end is minimal, Trenholm's work found strong impacts on service use and duration even a year after admission. Since the long-stay population makes up a significant portion of the Medicare home health population, it is important to understand whether this population will continue to be a significant part of home care, and how prospective payment may change their characteristics and service use.

We found that long-stay patients continued to be a significant part of the agencies' caseload, representing 6.5 percent of all admissions to prospectively paid agencies and 35 percent of all visits; and that these patients were concentrated in particular agencies. Surprisingly, this is about the same proportion we found in the control group. Thus, even though prospective payment significantly

reduced episode lengths, it did not reduce the importance of these long-stay patients. The characteristics we could observe of the long-stay patients remained the same--they were the same age, had the same primary diagnoses, and used the same amount of health care as the patients in cost-reimbursed agencies.

## B. LIMITATIONS OF THE ANALYSIS

Despite the significance and robustness of our impact findings on the overall population, this study has some limitations. Perhaps the most important potential limitation is the extent to which we can generalize our findings to home health agencies nationwide. Like any study design based on voluntary participation, the agencies in this demonstration may reflect the group best able (or most willing) to respond to the incentives of the intervention. If true, our demonstration impacts may overstate the actual reductions in service use that would take place nationally under a similar payment system. In addition, even if demonstration agencies had no particular predisposition to reduce the number of visits relative to similar agencies not participating in the demonstration, we may still overstate the impacts of prospective payment nationally because of other key differences between agencies in and out of the demonstration. For example, if agencies nationwide had relatively low-use practice patterns compared with demonstration agencies, we probably would overstate the effects of a nationwide program. Of course, if this difference were reversed, our impacts might actually understate these effects. Similarly, if control agencies were more responsive than agencies nationally to other pressures to contain or reduce their visits per episode, then our results might underestimate the national effects of a prospective payment system.

A key factor that suggests our results can be widely generalized is that agencies of *all types* made substantial reductions in their provision of services. Agencies of different size, auspice, or practice pattern might be expected to differ, both in their capacity and their desire to respond to

demonstration incentives. We find, however, that each agency type made cuts of at least 18 percent in their visits per patient. Thus, even if the mix of demonstration agencies differed significantly from agencies nationwide, we expect the impacts of a nationwide program to be substantial.

A second limitation, related to the issue of generalizability, is that the national program of prospective payment differs from the one implemented for the demonstration. For example, agencies are not be protected from incurring financial losses under a national system, which could lead some agencies to respond more cautiously to program incentives. Moreover, an agency's payment is not be based on its prior cost per episode. For many agencies, such as those with high-use practice patterns, this method could actually lead to larger cuts than were observed under the demonstration, because agencies might have to reduce their costs per episode substantially to remain solvent.

A third limitation is that it does not provide information on the consequences of reducing services. Previous reports have already examined the effects of the demonstration on the quality of care and patient outcomes, patient selection and retention, and the utilization of other Medicare services (Chen 2000; Trenholm 2000; and Schore 2000). A final report will combine the findings of this report on service use with the findings across other outcome measures.

A final concern is that we examined impacts over only a three-year period. While three years should be long enough for agencies to change their practices, our findings may not accurately reflect the long-term impacts of prospective payment. For the most part, however, the findings from this demonstration provide the information necessary to policymakers: within the first year of the demonstration, nearly all participating agencies in the treatment group made dramatic cuts in service use that may be attributed directly to the new payment system, and this continued over the course of the demonstration. Thus, a key hypothesis that the demonstration was designed to investigate--

whether prospective payment could curtail the tremendous growth in home health services--was strongly affirmed, despite the limited follow-up period.

## C. POLICY IMPLICATIONS

### 1. Implications of the Service Use Reductions

The results from this study further support the conclusion that a prospective payment system can be an effective mechanism to provide home health care efficiently. Combined with additional findings that suggest no reductions in the quality care under the demonstration (Chen, 2000), prospective payment appears to be a promising approach for HCFA to use in containing service use.

These findings also demonstrate the potential advantage of a payment system that provides an *ongoing* incentive to reduce service use. In every year of the demonstration, prospectively paid agencies reduced their visits significantly from the year before. Presumably, these reductions took place over a long period of time because agencies discovered and adopted new ways to reduce service use without harming the quality of care. This flexibility to improve care delivery, combined with the ongoing financial incentive to do so, contrasts sharply with a system of direct regulation (such as the interim payment system) that merely offers short-term incentives to meet established levels of savings. While results from this demonstration do not allow for a comparison of these two systems of payment, they do show that a system which provides an ongoing incentive, to improve care delivery will likely be successful.

### 2. Implications of the Analysis of Long-Term Stayers

One issue for designing the new national payment system is how to handle the long-stay patients in Medicare home health. Although, over the past decade, long-term stay patients used a large proportion of the Medicare home health resources, they conceivably could become a minor part of

the program once the financial incentives have changed. Although this demonstration does not offer as strong financial incentives as the national prospective payment system will, it does offer incentives to discharge patients early, and the prospectively paid agencies did so. We found, however, that, despite this incentive, long-stay patients remained about the same proportion of the caseload. Thus, as the national payment system is refined, attention to long-stay patients will continue to be an important issue.

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# APPENDIX A

TABLE A.1

NUMBER OF VISITS PROVIDED IN THE AT-RISK PERIOD, TOTAL AND BY SERVICE TYPE[a]

| | Year 1 | | | Year 2 | | | Year 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Number of Visits | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Change in Impacts Year 1-2[b] (p-value)[c] | Change in Impacts Year 2-3[b] (p-value)[c] |
| All | 37.6 | 45.3 | -7.7 (0.00) | 34.1 | 41.5 | -7.4 (0.00) | 32.0 | 41.0 | -9.0 (0.00) | 0.3 (0.78) | -1.6 (0.46) |
| Skilled Nursing | 18.0 | 21.7 | -3.7 (0.00) | 17.2 | 19.8 | -2.6 (0.03) | 15.6 | 20.8 | -5.2 (0.00) | 1.1 (0.15) | -2.6 (0.06) |
| Home Health Aide | 12.6 | 15.9 | -3.3 (0.00) | 10.2 | 14.0 | -3.8 (0.00) | 9.5 | 12.3 | -2.8 (0.02) | -0.5 (0.54) | 1.0 (0.38) |
| Physical Therapy | 5.5 | 5.7 | -0.2 (0.72) | 5.1 | 5.6 | -0.5 (0.34) | 5.4 | 5.9 | -0.5 (0.46) | -0.3 (0.27) | 0.0 (0.88) |
| Occupational Therapy | 0.8 | 1.0 | -0.2 (0.22) | 0.7 | 1.0 | -0.3 (0.13) | 0.9 | 1.0 | -0.1 (0.47) | -0.1 (0.47) | 0.2 (0.22) |
| Speech Therapy | 0.3 | 0.3 | 0.0 (0.39) | 0.3 | 0.3 | 0.0 (0.95) | 0.3 | 0.3 | 0.0 (0.55) | 0.0 (0.40) | 0.0 (0.56) |
| Medical Social Worker | 0.5 | 0.8 | -0.3 (0.01) | 0.5 | 0.8 | -0.3 (0.01) | 0.5 | 0.8 | -0.3 (0.00) | 0.0 (0.60) | 0.0 (0.17) |
| Sample Size | | | | | | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 20,359 | 19,274 | ----- | 12,451 | 11,641 | ----- | ----- | ----- |
| Agencies | 36 | 38 | | 36 | 38 | | 36 | 38 | | | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Regression-adjusted (through ordinary least squares models) to control for preexisting differences between treatment and control agencies.

[c] The p-value corresponds to a test of whether the treatment-control difference (impact) is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

TABLE A.2

CUMULATIVE NUMBER OF VISITS PROVIDED IN THE AT-RISK PERIOD[a]

| | Year 1 | | | Year 2 | | | Year 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Total Visits from Admission Through: | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Change in Impacts Year 1-2[b] (p-value)[c] | Change in Impacts Year 2-3[b] (p-value)[c] |
| 30 days | 17.6 | 19.6 | -2.0 (0.00) | 16.6 | 18.5 | -1.9 (0.01) | 16.3 | 19.1 | -2.8 (0.00) | 0.1 (0.79) | -0.9 (0.20) |
| 60 days | 27.2 | 31.3 | -4.1 (0.00) | 25.0 | 29.1 | -4.1 (0.00) | 24.3 | 30.0 | -5.7 (0.00) | 0.0 (0.99) | -1.6 (0.20) |
| 90 days | 33.0 | 39.0 | -6.0 (0.00) | 30.1 | 35.9 | -5.8 (0.00) | 28.6 | 36.3 | -7.7 (0.00) | 0.2 (0.85) | -1.9 (0.00) |
| 120 days | 37.6 | 45.3 | -7.7 (0.00) | 34.1 | 41.5 | -7.4 (0.00) | 32.0 | 41.0 | -9.0 (0.00) | 0.3 (0.78) | -1.6 (0.46) |
| Sample Size | | | | | | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 20,359 | 19,274 | ----- | 12,451 | 11,641 | ----- | ----- | ----- |
| Agencies | 36 | 38 | | 36 | 38 | | 36 | 38 | | | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Regression-adjusted (through ordinary least squares models) to control for preexisting differences between treatment and control agencies.

[c] The p-value corresponds to a test of whether the treatment-control difference (impact) is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

TABLE A.3

PROBABILITY OF RECEIVING A GIVEN SERVICE DURING THE AT-RISK PERIOD, BY TREATMENT STATUS [a]

| | Year 1 | | | Year 2 | | | Year 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Service | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Change in Impacts Year 1-2 [b] (p-value) [c] | Change in Impacts Year 2-3 [b] (p-value) [c] |
| Skilled Nursing Visit(s) | 92.6 | 96.6 | -4.0 (0.01) | 91.7 | 95.2 | -3.5 (0.02) | 90.9 | 93.7 | -2.8 (0.05) | 0.5 (0.54) | 0.7 (0.42) |
| Home Health Aide Visit(s) | 44.4 | 46.7 | -2.3 (0.18) | 39.3 | 43.7 | -4.4 (0.04) | 39.9 | 42.4 | -2.5 (0.24) | -2.1 (0.10) | 1.9 (0.23) |
| Any Therapy Visit(s) | 43.5 | 44.5 | -1.0 (0.70) | 44.4 | 45.1 | -0.7 (0.80) | 48.0 | 49.8 | -1.8 (0.51) | 0.3 (0.82) | -1.1 (0.46) |
| Physical Therapy Visit(s) | 42.6 | 42.6 | 0.0 (0.99) | 43.4 | 43.0 | 0.4 (0.89) | 46.9 | 47.8 | -0.9 (0.73) | 0.4 (0.82) | -1.3 (0.38) |
| Occupational Therapy Visit(s) | 7.7 | 12.5 | -4.8 (0.01) | 8.1 | 12.8 | -4.7 (0.04) | 9.2 | 13.9 | -4.7 (0.07) | 0.1 (0.70) | 0.0 (0.37) |
| Speech Therapy Visit(s) | 2.7 | 3.3 | -0.6 (0.08) | 2.7 | 3.2 | -0.5 (0.24) | 2.4 | 3.4 | -1.0 (0.20) | 0.1 (0.40) | -0.5 (0.70) |
| Medical Social Worker Visit(s) | 25.8 | 29.2 | -3.4 (0.21) | 23.3 | 28.2 | -4.9 (0.1) | 19.0 | 29.3 | -10.3 (0.01) | -1.5 (0.59) | -5.4 (0.03) |
| Sample Size | | | | | | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 20,359 | 19,274 | ----- | 12,451 | 11,641 | ----- | ----- | ----- |
| Agencies | 36 | 38 | | 36 | 38 | | 36 | 38 | | | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Regression-adjusted (through logit models) to control for preexisting differences between treatment and control agencies.

[c] The p-value corresponds to a test of whether the treatment-control difference (impact) is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

TABLE A.4

CUMULATIVE PERCENTAGE OF EPISODES ENDED IN EACH MONTH OF THE AT-RISK PERIOD, BY TREATMENT STATUS[a]

| | Year 1 | | | Year 2 | | | Year 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Time Since Admission | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Change in Impacts Year 1-2[b] (p-value)[c] | Change in Impacts Year 2-3[b] (p-value)[c] |
| 0-30 days | 33.0 | 26.6 | 6.4 (0.00) | 36.4 | 30.3 | 6.1 (0.00) | 40.4 | 31.7 | 8.7 (0.00) | -0.3 (0.78) | 2.6 (0.08) |
| 31-60 days | 56.8 | 49.2 | 7.6 (0.00) | 61.3 | 53.9 | 7.4 (0.00) | 68.9 | 58.8 | 10.1 (0.00) | -0.2 (0.83) | 2.7 (0.11) |
| 61-90 days | 66.3 | 58.8 | 7.5 (0.00) | 70.2 | 64.0 | 6.2 (0.00) | 77.3 | 70.5 | 6.8 (0.00) | -1.3 (0.26) | 0.6 (0.78) |
| 91-120 days | 74.0 | 66.3 | 7.7 (0.00) | 78.6 | 72.3 | 6.3 (0.00) | 86.0 | 79.5 | 6.5 (0.00) | -1.4 (0.21) | 0.2 (0.96) |
| Sample Size | | | | | | | | | | | |
| Patients | 26,528 | 23,863 | ----- | 20,359 | 19,274 | ----- | 12,451 | 11,641 | ----- | ----- | ----- |
| Agencies | 36 | 38 | | 36 | 38 | | 36 | 38 | | | |

SOURCE: Medicare claims data.

NOTE: Individual observations (patients) weighted to give agencies equal representation in the calculations.

[a] Reflects a patient's first admission to a home health agency participating in the demonstration.

[b] Means for treatment agencies have been regression-adjusted (through logit models) to control for preexisting differences between patients or agencies.

[c] The p-value corresponds to a test of whether the treatment-control difference (impact) is statistically significant from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

TABLE A.5

IMPACT OF PROSPECTIVE PAYMENT ON SERVICE USE, BY WHETHER THE AGENCY HAS

A HIGH-USE OR LOW-USE PRACTICE PATTERN[a]

| | Year 1 | | | Year 2 | | | Year 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Change in Impacts Year 1-2[b] (p-value)[c] | Change in Impacts Year 2-3[b] (p-value)[c] |
| **Service Use During the At-Risk Period** | | | | | | | | | | | |
| Total Visits | | | | | | | | | | | |
| High-volume practice pattern | 41.6 | 52.5 | -10.9*** | 37.8 | 47.0 | -9.2*** | 36.4 | 47.1 | -10.7*** | 1.7 | -1.5 |
| Low-volume practice pattern | 28.9 | 31.6 | -2.7*** | 25.5 | 30.9 | -5.4** | 22.0 | 29.4 | -7.4*** | -2.7* | -2.0 |
| Subgroup difference | | | -8.2 (0.01) | | | -3.8 (0.26) | | | -3.3 (0.37) | 4.4 (0.04) | 0.5 (0.89) |
| Skilled Nursing Visits | | | | | | | | | | | |
| High-volume practice pattern | 19.4 | 25.1 | -5.7*** | 18.6 | 22.7 | -4.1** | 17.1 | 24.3 | -7.2*** | 1.6* | -3.1 |
| Low-volume practice pattern | 14.3 | 15.0 | -0.7 | 13.7 | 14.2 | -0.5 | 11.5 | 14.1 | -2.6** | 0.2 | -2.1 |
| Subgroup difference | | | -5.0 (0.01) | | | -3.6 (0.13) | | | -4.6 (0.08) | 1.4 (0.33) | -1.0 (0.69) |
| Home Health Aide Visits | | | | | | | | | | | |
| High-volume practice pattern | 14.7 | 19.7 | -5.0*** | 12.0 | 17.1 | -5.1*** | 11.6 | 15.2 | -3.6 | 0.1 | 1.5 |
| Low-volume practice pattern | 8.2 | 8.5 | -0.3 | 6.0 | 8.2 | -2.2* | 4.5 | 6.7 | -2.2 | -1.9*** | 0.0 |
| Subgroup difference | | | -4.7 (0.00) | | | -2.9 (0.09) | | | -1.4 (0.51) | 1.8 (0.20) | 1.5 (0.38) |
| Physical Therapy Visits | | | | | | | | | | | |
| High-volume practice pattern | 5.6 | 5.5 | 0.1 | 5.5 | 5.4 | 0.1 | 5.8 | 5.7 | 0.1 | 0.0 | 0.0 |
| Low-volume practice pattern | 5.3 | 6.0 | -0.7 | 4.8 | 6.2 | -1.4** | 5.0 | 6.4 | -1.4* | -0.7** | 0.0 |
| Subgroup difference | | | 0.8 (0.44) | | | 1.5 (0.18) | | | 1.5 (0.23) | 0.7 (0.21) | 0.0 (0.99) |
| Probability of Last | | | | | | | | | | | |
| High-volume practice pattern | 72.4 | 62.3 | 10.1*** | 77.1 | 69.3 | 7.8*** | 85.4 | 77.1 | 8.3*** | -2.4 | 0.6 |
| Low-volume practice pattern | 78.1 | 74.2 | 3.9** | 82.0 | 77.9 | 4.1 | 88.5 | 84.1 | 4.4 | 0.2 | 0.3 |
| Subgroup difference | | | 6.2 (0.06) | | | 3.7 (0.33) | | | 3.9 (0.33) | -2.6 (0.34) | 0.3 (0.90) |

TABLE A.5 *(continued)*

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] An agency is defined as having a high-use practice pattern if its (case mix-adjusted) number of visits per episode in the base (predemonstration) period was above the median for all agencies in the sample. Otherwise it is defined as having a low-use practice pattern.

[b] Reflects a patient's first admission to a home health agency participating in the demonstration.

[c] Regression-adjusted (through ordinary least squares and logit models) to control for preexisting differences between treatment and control agencies.

[d] The p-value corresponds to a test of whether the subgroup difference is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.6

IMPACT OF PROSPECTIVE PAYMENT ON SERVICE USE, BY SMALL OR LARGE AGENCY SIZE[a]

| | Year 1 | | | Year 2 | | | Year 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Change in Impacts Year 1-2[b] (p-value)[c] | Change in Impacts Year 2-3[b] (p-value)[c] |
| **Service Use During the At-Risk Period** | | | | | | | | | | | |
| Total Visits | | | | | | | | | | | |
| Small size | 52.7 | 60.2 | -7.5** | 44.4 | 52.4 | -8.0* | 41.5 | 50.5 | -9.0* | -0.5 | -1.0 |
| Large size | 40.0 | 42.0 | -2.0*** | 34.5 | 39.0 | -4.5*** | 32.8 | 38.9 | -6.1*** | -2.5 | -1.6 |
| Subgroup difference | | | -5.5 | | | -3.5 | | | -2.9 | 2.0 | 0.6 |
| | | | (0.35) | | | (0.58) | | | (0.65) | (0.58) | (0.93) |
| Skilled Nursing Visits | | | | | | | | | | | |
| Small size | 26.3 | 25.6 | 0.7 | 24.8 | 23.4 | 1.4 | 23.8 | 24.5 | -0.7 | 0.7 | -2.1 |
| Large size | 19.7 | 20.8 | -1.1*** | 17.6 | 18.9 | -1.3*** | 17.2 | 20.0 | -2.8** | -0.2 | -1.5* |
| Subgroup difference | | | 1.8 | | | 2.7 | | | 2.1 | 0.9 | -0.6 |
| | | | (0.59) | | | (0.49) | | | (0.59) | (0.72) | (0.89) |
| Home Health Aide Visits | | | | | | | | | | | |
| Small size | 21.2 | 26.8 | -5.6*** | 16.2 | 22.3 | -6.1** | 14.3 | 16.8 | -2.5 | -0.5 | 3.6 |
| Large size | 14.0 | 13.4 | 0.6* | 11.1 | 12.2 | -1.1*** | 9.9 | 11.2 | -1.3 | -1.7* | -0.2 |
| Subgroup difference | | | -6.2 | | | -5.0 | | | -1.2 | 1.2 | 3.8 |
| | | | (0.04) | | | (0.17) | | | (0.68) | (0.69) | (0.31) |
| Physical Therapy Visits | | | | | | | | | | | |
| Small size | 4.8 | 5.8 | -1.0 | 3.5 | 4.5 | -1.0 | 3.4 | 6.6 | -3.2** | 0.0 | -2.2* |
| Large size | 5.0 | 5.7 | -0.7 | 4.7 | 5.9 | -1.2 | 4.7 | 5.7 | -1.0 | 0.5 | 0.2 |
| Subgroup difference | | | -0.3 | | | 0.2 | | | -2.2 | 0.5 | -2.4 |
| | | | (0.83) | | | (0.85) | | | (0.11) | (0.46) | (0.03) |
| **Probability of Last Visit by Day 120** | | | | | | | | | | | |
| Small size | 66.8 | 56.8 | 10.0*** | 70.0 | 65.7 | 4.3 | 83.9 | 76.7 | 7.2 | -5.7* | 2.9 |
| Large size | 75.3 | 68.5 | 6.8*** | 80.7 | 73.7 | 7.0*** | 86.2 | 80.1 | 6.1*** | 0.2 | -0.8 |
| Subgroup difference | | | 3.2 | | | -2.7 | | | 1.1 | -5.9 | 3.7 |
| | | | (0.47) | | | (0.50) | | | (0.78) | (0.09) | (0.37) |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[a] An agency is defined as large (small) if it provided more (fewer) than 30,000 visits during the predemonstration year.

[b] Reflects a patient's first admission to a home health agency participating in the demonstration.

TABLE A.6 *(continued)*

[c] Regression-adjusted (through ordinary least squares and logit models) to control for preexisting differences between treatment and control agencies.

[d] The p-value corresponds to a test of whether the subgroup difference is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE A.7

IMPACT OF PROSPECTIVE PAYMENT ON SERVICE USE, BY PROFIT STATUS[a]

| | Year 1 | | | Year 2 | | | Year 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Treatment Group Mean[b] | Control Group Mean | Difference[b] (p-value)[c] | Change in Impacts Year 1-2[b] (p-value)[c] | Change in Impacts Year 2-3[b] (p-value)[c] |
| **Service Use During the At-Risk Period** | | | | | | | | | | | |
| Total Visits | | | | | | | | | | | |
| For-profit | 49.2 | 51.8 | -2.6*** | 41.6 | 47.9 | -6.3*** | 30.7 | 45.3 | -14.6*** | -3.7 | -8.3 |
| Nonprofit | 35.7 | 38.2 | -2.5*** | 29.4 | 34.2 | -4.8*** | 23.3 | 27.9 | -4.6* | -2.3 | 0.2 |
| Subgroup difference | | | -0.1 | | | -1.5 | | | -10.0 | -1.4 | -8.5 |
| | | | (0.97) | | | (0.70) | | | (0.03) | (0.53) | (0.05) |
| Skilled Nursing Visits | | | | | | | | | | | |
| For-profit | 24.1 | 25.5 | -1.4*** | 23.0 | 23.1 | -0.1 | 16.0 | 23.9 | -7.9 | 1.3 | -7.8*** |
| Nonprofit | 17.1 | 17.4 | -0.3*** | 15.4 | 16.0 | -0.6** | 13.0 | 13.3 | -0.3 | -0.3 | 0.3 |
| Subgroup difference | | | -1.1 | | | 0.5 | | | -7.6 | 1.6 | -8.1 |
| | | | (0.54) | | | (0.81) | | | (0.00) | (0.36) | (0.00) |
| Home Health Aide Visits | | | | | | | | | | | |
| For-profit | 17.2 | 18.5 | -1.3*** | 12.5 | 17.0 | -4.5*** | 8.6 | 13.7 | -5.1*** | -3.2 | -0.6 |
| Nonprofit | 13.3 | 13.0 | 0.3 | 9.7 | 10.7 | -1.0 | 6.8 | 7.7 | -0.9 | -1.3 | 0.1 |
| Subgroup difference | | | -1.6 | | | -3.5 | | | -4.2 | -1.9 | -0.7 |
| | | | (0.46) | | | (0.12) | | | (0.10) | (0.31) | (7.6) |
| Physical Therapy Visits | | | | | | | | | | | |
| For-profit | 6.4 | 5.5 | 0.9 | 5.1 | 5.6 | -0.5 | 5.1 | 5.5 | -0.4 | -1.4 | 0.1 |
| Nonprofit | 4.5 | 5.9 | -1.4** | 3.8 | 5.6 | -1.8* | 3.4 | 5.4 | -2.0* | -0.4 | -0.2 |
| Subgroup difference | | | 2.3 | | | 1.3 | | | 1.6 | -1.0 | 0.3 |
| | | | (0.02) | | | (0.17) | | | (0.14) | (0.11) | (0.60) |
| **Probability of Last Visit by Day 120** | | | | | | | | | | | |
| For-profit | 71.7 | 63.2 | 8.5*** | 76.0 | 68.9 | 7.1*** | 87.0 | 74.6 | 12.4*** | -1.5 | 5.4* |
| Nonprofit | 76.3 | 69.9 | 6.4*** | 81.2 | 76.0 | 5.2** | 85.4 | 84.9 | 0.5 | -1.2 | -4.6* |
| Subgroup difference | | | 2.1 | | | 1.9 | | | 11.9 | -0.2 | 10.0 |
| | | | (0.52) | | | (0.69) | | | (0.01) | (0.85) | (0.02) |

SOURCE: Medicare claims data.

NOTE: Individual observations (patient records) have been weighted to give agencies equal representation in the calculations.

[b]Reflects a patient's first admission to a home health agency participating in the demonstration.

[c] Regression-adjusted (through ordinary least squares and logit models) to control for preexisting differences between treatment and control agencies.

TABLE A.7 *(continued)*

[a]The p-value corresponds to a test of whether the subgroup difference is statistically different from zero. It is based on standard errors inflated to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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